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and these, be it remembered, are not light duties, for they are not merely those which you see here performed, but there are other arduous ones connected with the management of the Society; so that I ask you to go with me in proposing a hearty vote of thanks to the late President, Lieut.-Col. Champain, for the efficient manner in which he has fulfilled his duties during the past year.

Sir CHARLES BRIGHT, in seconding the proposition wished to remind the Members that, although the retiring President was an officer of the Royal Engineers, he was almost, if not quite as much a Telegraph Engineer as any of the professional men among the Members, seeing that he had been engaged in carrying out telegraph works for perhaps twenty years. It was his fortune to be connected with the construction of the first line of telegraphs to India, as also with various other lines in Asia and Europe. These remarks would show that although the gallant President was worthy the honours shown to him by Sir Robert Napier for military deeds, he was also deserving to be recognised as a thoroughly practical Telegraph Engineer, and as such had well filled the Chair for the past year, for which the Society would tender him their warmest thanks.

Lieut.-Colonel BATEMAN-CHAMPAIN, R.E.: Gentlemen,—I wish I could feel that I deserve one quarter of what Professor Abel and Sir Charles Bright have stated about me. All that I can say is, that I repeatedly felt that the post was very much above me, and I retire feeling quite sure that the chair will be ably filled during the coming year by Mr. Preece.

# INAUGURAL ADDRESS.

By Mr. William Henry Preece (*President*).

*January*, 28, 1880.

On surveying the wide sea upon which the numerous and varied practical applications of Electricity are launched for the subject of this evening's address, I have been puzzled to steer a course that shall avoid the dazzling shoals of theory on the one hand, and the dry hard rocks of practice on the other. Hypothesis is a veritable Scylla that captivates the imagination and often sends the visionary to destruction, while practice alone is a hard-hearted Charybdis that lures the matter-of-fact practical man to folly and expense. Practice must be tempered with theory to utilize advantageously the great forces of nature, and theory itself must be based on practice, or on facts, to be comprehensive and acceptable. Hence success is the offspring of the marriage of practice and theory, and, therefore, as the two are so intimately connected, I have determined to steer a middle course to-night to survey the progress of each in our profession, and to show their mutual relationship.

What is theory? It is an explanation of the hidden cause of certain effects that are evident to the senses. It is an effort of the imagination to account for operations that are in themselves invisible and insensible, but which result in facts that are observable and known. Thus the movements of all those bright bodies by which

> "the floor of heaven
> Is thick inlaid with patines of bright gold,"

are explained by the theory of gravity. Their appearance, vagaries, and beauties are accounted for by the undulatory theory of light. The warmth that the monarch of them all shed upon this earth countless ages ago, and that is now restored to us in our household fires, is explicable on the molecular theory of heat. The constitution of matter and its various states of solid, liquid, and gas, are completely explained by the atomic theory of Democritus and Dalton, and the modern kinetic theory of gases.

It is impossible for a practical man who has devoted more than a quarter of a century to *the application* of electricity to useful

purposes, to avoid devoting much contemplation to the nature of the agent which he has to make use of. The imagination cannot be checked. The mind will dream and theorise. The untutored savage fancies he hears in the roar of the thunder the anger of the Great Spirit, and the tutored Greek saw in the daily course of the sun the chariot wheels of his god Helios, and in the grey dawn of the morning the advent of the soft and gentle Aurora. The Saxon churl saw in the lambent flame of the phosphuretted hydrogen of his marshes the gambolings of Jack o' Lantern, or the treacherous ways of Will o' the Wisp. Is there a member of this society who has not striven to peer into the region of the unknown, who has not speculated on the power he uses, or who has not formed some conception in his mind of the nature of electricity? Yet it is remarkable that the answer to the question, What is electricity? cannot even now be given with authority. Faraday, our great apostle, whose researches should be every Electrician's bible, declined to venture an answer, nor did he ever directly formulate his ideas on the subject, though his publications indicate pretty clearly and with no uncertain sound what they were. Clerk-Maxwell, who, while he overthrew all existing theories, failed to supply their place before he was so untimely removed from us. Sir William Thomson, in his published papers, always carefully eschews the consideration of any physical theory of electricity. The French Electricians simply use the one-fluid theory as a convenience of language, while the Germans, as a rule, employ the two-fluid theory merely for mathematical purposes. Hence there is no recognised theory of electricity. Some maintain, with Du Fay or with Franklin, that it is a form of matter—a substance; others, following Faraday and Grove, consider it a form of force—a motion—like heat and light. It must be either one or the other. There is no other category in which to class it. If it is not a form of matter it must be a form of force. The question I purpose to discuss is, therefore, Is electricity a form of matter, or is it a form of force?

In discussing such a vexed question it is necessary to be very precise in language to avoid any misconception of my meaning, therefore I will define both matter and force in the sense in which I use those terms. *Matter* is that which can be perceived by the

senses, or can be acted upon by force. It is characterised by weight, inertia, and elasticity. *Force* is that which produces, or tends to produce, the motion of matter. It may be pressure, tension, attraction, repulsion, or any thing capable of causing alteration in the natural state of rest or of existing motion of matter.

Matter consists of sixty-four known elements which have not yet been decomposed by any known means. There may be other elements that have not yet been discovered by chemists, but they must exist in some known compound. Matter is found in either the solid, liquid, gaseous, or ultra-gaseous state, and it occupies space. It consists of atoms and molecules. The *atom* is the smallest indivisible part of an element, and a group of atoms of the same or of different elements forms the *molecule*, which has a definite magnitude and is unalterable in form for each substance. The *mass* of a substance is the aggregate of the molecules of which it is composed. There is no generation or destruction of atoms. The indestructibility of matter is a fixed law in nature. The size of the molecule is approximately known. Sir William Thomson says :—"If we conceive a sphere of water as large as a pea to be magnified to the size of the earth, each molecule being magnified to the same extent, the magnified structure would be coarser-grained than a heap of small lead shot, but less coarse-grained than a heap of cricket balls." Fifty million molecules ranged in single file would occupy an inch. They are highly elastic, and unless interfered with would move with constant velocity in straight lines. When they can move about freely without interfering with each other's proceedings, we have the *ultra-gaseous* state of Crookes, a state found only in very high vacua and under certain adventitious circumstances. When they collide and impinge on each other according to the law of the impact of elastic bodies, interfering with each other's path, we have *gases* as we know them; when their mean free path is so reduced as to bring them within the sphere of mutual attraction, without too narrowly restricting their play, we have *liquids ;* when the attraction becomes cohesion and the motion of the molecule is confined to its own sphere, we have solids. The

number of molecules in a given volume of gas is known, and their velocity calculated. In hydrogen the velocity at 0° Cent. is 6,097 feet per second, the number being $10^{23}$ per cubic inch. The mean free path of a molecule in air at ordinary pressures is the ten-thousandth part of a millimetre. Besides their constant motion in straight lines the molecules may be set in vibration, rotation, or any other kind of relative motion whatever.

This is the atomic theory of matter born in the brain of Democritus "the laughing philosopher" 2,300 years ago; preached by Epicurus in Athens, and taught by Lucretius in Rome before the Christian era; lying dormant for eighteen centuries, until it was formulated by Dalton in the last century, and removed from the region of pure speculation by Joule, Clausius, Clerk-Maxwell, and Crookes during our days.

The definition of force shows us that whatever changes or tends to change the motion of matter (or of the molecules of which it is composed), by altering either its direction or its magnitude, is a form of force. Thus gravity is a form of force, for it attracts all matter to the centre of the earth, and it is measured by the rate per second at which a body acquires a velocity in this direction when falling freely at a given spot. Heat is a form of force, for it throws the molecules of matter into violent vibration, or it increases the velocity of their motion in straight lines, which thus becomes the measure of its heat or its *temperature*. Light is a form of force, for it is produced by the undulation of the molecules of matter, and it is transmitted by the undulations of that medium called Ether, which fills all space.

No man has seen or can see a molecule, nor have we any objective idea of what force really is. When we attempt to reach beyond these definitions, we tread upon the threshold of the holy of holies, on whose confines only are we permitted to dwell, and into which we are not yet allowed to enter. "Hitherto shalt thou come but no further, and here shall thy proud waves be stayed." Let us, therefore, be content with precise definitions and clear mental conceptions, speculative though they be, of matter inert and of matter in motion. The *ultima thule* of the scientific man is theory, and at any time his most cherished notions may be

"Melted into air, into thin air."

The scientific man, while rather too fond of decrying the exercise of faith in others, is himself the humblest slave of the imagination. A physical theory may be complete. The various facts and laws which it embraces may be related mathematically with one another, yet one single incompatible phenomenon may dissolve it,

> "And, like this insubstantial pageant faded,
> Leave not a rack behind."

Hence, while apparently dogmatic in my description of the present theory of matter and force, I wish it distinctly to be understood that it rests, and must continue to rest, on the imaginative power of the mind.

Nevertheless, subjective speculation may become objective reality. Who is there that doubts the existence of the ether filling all space, transmitting those exquisite and delicate vibrations which impart to us the sensation of light and heat from stars and nebulæ countless millions of miles away? And with regard to the atomic structure of matter, though but a speculation as yet, it is so complete, so apt, and so thorough, that for my part, I have not the slightest doubt of its reality, and therefore I submit it with apparent dogmatism.

When we take a given free mass and impress upon it a given force, we throw that mass into motion; for instance, when we fire a loaded cannon, we have imparted to the ball "*energy*," and in virtue of the motion of the ball, this energy is called "*kinetic*." Again, if we lift the ball to a certain height above the earth's surface—say to the top of a tower—and let it remain there, we have again imparted to it "energy," but this time it is called "*potential*," for it is dormant or resting. In each case the energy possessed by the ball is the exact equivalent of the work done upon it, that is, of the force impressed and the distance through which it has acted. The motion of the ball is readily transferred to the motion of the individual molecules of the ball. When, in the first case adduced, the ball strikes the side of a ship or a target, its kinetic energy is thus converted into light and heat, which is molecular motion; or, in the second case, when it is allowed to fall, its potential energy is converted into kinetic energy, which again, on coming in contact with the ground, is converted into molecular

motion or heat. Energy is always either potential or kinetic, and one of the most remarkable generalizations of modern days is the grand principle of the conservation of energy, which implies that the total energy of the universe is a quantity which can neither be increased nor diminished, though it may be transformed into any of the forms of which energy is susceptible. Energy is therefore as indestructible as matter. All the recent advances in the science of heat have been due to the discovery of this principle, and its application to electricity has gone far to remove that science from the hypothetical state in which it has existed so long.

My purpose is to contend that electricity is not a form of matter but a form of force, and that all its effects are evident to us in one or other of the several forms of energy characterised by the motions of molecules or of mass.

It is interesting to trace the historical growth of theories. The uncultivated human intellect cannot soar above its own limited sphere of childish observation. Whatever is mysterious and incomprehensible in nature is attributed to that which is equally mysterious and incomprehensible. Life has ever been of this character, and heat, magnetism, electricity, and many other unaccountable physical phenomena, have each in their turn been supposed to be cases of life. Even now there are those who would attribute exceptional and peculiar phenomena to spiritual agencies.

Heat was thought by the Greeks to be an animal that bit. It was then for many centuries thought to be a fluid which, entering into bodies, like mercury, made them swell, and this idea existed until this generation, when Rumford showed it to be a kind of motion, and Joule made it a quantitative form of energy.

Thales, of Miletus, thought that the magnet was endowed with a sort of immaterial spirit, and to possess a species of animation. The Greeks knew also that rubbed amber attracted bits of straw, and supposed it to be endowed with life. Even Boyle, as late as 1675, imagined it to emit a sort of glutinous effluvium which laid hold of small bodies and pulled them towards the excited body. Du Fay in 1733 conceived the double fluid theory, and Franklin in 1747 invented the single fluid theory. Cavendish in 1771

supplied some of the deficiencies of Franklin's theory, but it was Faraday who first exploded the fluid notion and originated the molecular theory of electricity, while Grove boldly classed electricity with light and heat as correlated forces and mere modes of motion.

Light was thought by the Platonists to be the consequence of something emitted from the eye meeting with certain emanations from the surface of things, but no theory of light properly so called was attempted until Newton produced his celebrated corpuscular theory in 1670, which has lasted until the present day. Even as late as 1816 Faraday himself said—"The conclusion that is now generally received appears to be, that light consists of minute atoms of matter of an octahedral form, possessing polarity, and varying in size or in velocity."* Although Huyghens in Newton's own time conceived the undulatory theory, the superior authority of the great English philosopher overshadowed the lesser light, and it was not until Young and Fresnel at the commencement of this century took the matter up, that the present theory of light took firm root.

Thus we see that all these sciences have passed through the same stages of mystery and fancy, and it is only within the present generation that they have emerged from the mythical to the natural, from mere hypothesis to true theory. *Hypothesis* is an imaginary explanation of the cause of certain phenomena which remains to be shown probable or to be proved true. *Theory* is this supposition when it has been shown to be highly probable and all known facts are in agreement with its truth.

A theory, therefore, to be valid and true, must agree with every observed fact; it must not conflict with natural laws; it must suggest new experience, and it should lead to further developments. A theory is absurd if it supposes an agent to act in a manner unknown in all other cases. The fluid theories of electricity are merely descriptive, they do not agree with every observed fact; they have never prompted the invention of a single new experiment, or led to any development. They suppose an

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* *Life, vol. i., page 216.*

agent unknown in other cases and opposed to natural laws. Incomplete theories die a natural death, thus Descartes' vortices, Newton's corpuscular theory of light, the fluid theory of heat, Stahl's phlogiston, Nature abhorring a vacuum, have all disappeared, while complete theories, such as that of gravity, the laws of motion, the conservation of energy, the undulatory theory of light, not only remain, but suggest new fields of enquiry, open out fresh pastures, carry truth and conviction with them, and have led to the most wonderful predictions. The fluid theories of electricity are certainly incomplete, and they deserve a speedy interment. We have to assume the existence of two substances of opposite qualities which mutually annihilate each other on combination —a self-evident absurdity, for the conception of matter involves indestructibility. Franklin imagined his one fluid to be an element of glass; remove electricity, and glass would lose its virtues and properties, and thus glass was to give out its electricity for ever and a day, without loss of weight or sensible diminution. It was to be devoid of dimensions, inertia, weight, and elasticity, and is therefore outside the pale of our definition.

Electricity is therefore not a form of matter. Hence, according to our reasoning, it must be a form of force.

But can we not prove that it is a form of force? Certainly.

Let us first argue from analogy. We know that sound, heat, and light are modes of motion, in what respects does electricity agree with these forms of force?

The fundamental law of electrostatics is that two bodies charged with opposite electricities attract each other with a force dependent on the square of the distance separating them. Whatever influence or power spreads from a point and expands uniformly through space varies in intensity as the square of the distance for the area over which it is spread increases as the square of the radius. This is the case with gravity, light, sound, and heat, which are known forms of force. It is also the case with electricity and magnetism, which ought therefore to be similar forms of force.

If we regard the velocity of transmission of certain electrical disturbances through space, we have every reason to believe that it is the same as that of radiant heat and light. In 1859 two observers in

different parts of the country (Messrs. Carrington and Hodgson) saw simultaneously a bright spot break out on the face of the sun, whose duration was only five minutes. Exactly at this time the magnetic needles at Kew were jerked, and the telegraph wires all over the world were disturbed. Telegraphists were shocked, and an apparatus in Norway was set on fire. Auroras followed, and all the effects of powerful magnetic storms. Moreover, the periods of sun spots, earth currents, and magnetic storms follow the same cycle of about eleven years. Dr. Hopkinson, among others, has shown that this electric disturbance through space is as mechanical as its action through short distances, and is therefore analogous with the ordinary strains of elastic matter subject to distortion by mechanical force. But Clerk-Maxwell has gone beyond this, and has shown that the velocity of light is identical with that of the propagation of electrical disturbances through space as well as through air and other transparent media. Hence, as light is admitted to be a mode of motion identical with radiant heat, electricity must be of the same category.

There is such a remarkable analogy between the conductivity of the different metals for heat and for electricity—indeed, there is every reason to believe that if the metals were pure, the order and ratio of conductivity would be identical—that it is impossible to resist the conclusion that the mode of transmission in each case is the same. Mr. Chandler Roberts, who, using Prof. Hughes' beautiful induction balance, shewed, by experiments on a comprehensive series of alloys, that the curves indicating the induction-balance effect closely resemble their curves of electrical resistance. He was also able to demonstrate that the induction-balance curve of the copper-tin alloys is almost identical with the curve of the conductivity of heat:—a conclusion of much interest; and he pointed out that we might look with confidence to being able to ascertain, by the aid of the induction-balance, whether the relation between the conductivity of heat and electricity is really as simple as it has hitherto been supposed to be. Moreover, when a wire conveys a current of electricity it is warmed, as the strength of current is increased it is heated and eventually rendered incandescent. The ultimate form which every electric current takes is heat. The wire of every telegraph is warmed in proportion to the

currents it transmits. Joule showed that when this heat is produced by a current generated in a battery by chemical force, its amount is exactly equivalent to that which would have been evolved by the direct combination of the atoms. The conducting power of all bodies is affected by heat, and some even, like selenium, by light. Hence, as we know that, in the case of heat and light, conduction is molecular vibration, we reasonably conclude that it is the same with electricity. In fact, it is impossible to account for these phenomena except on the assumption of the motion of the molecules.

The magnificent researches of Dr. Warren de la Rue and Dr. Hugo Müller on the electric discharge with the 11,000 cells of chloride of silver battery that the former philosopher has provided himself with in his celebrated laboratory, have shown indisputably that the discharge in air or in gases under various pressures is a function of the molecules filling the space through which the discharge occurs. In fact, the resistance of the discharge between parallel flat surfaces is as the number of molecules intervening between them; and they show that during electrical discharge in a gas there is a sudden and considerable pressure produced by a projection of the molecules against the sides of the containing vessel distinct from that caused by heat, and unquestionably due to the molecular action of electrification. The long-continued and patient researches which these eminent physicists are carrying out prove beyond doubt that electrical discharge is simply molecular disturbance. In reality, the fact that no discharge occurs through a perfect vacuum is a crucial proof of the molecular theory.

Some recent very remarkable researches of M. Planté with his rheostatic machine* have shown that fine wires conveying powerful currents are wrinkled up into well defined regular nodes, that these effects are accompanied by a peculiar crackling, and that the wire itself becomes brittle, giving clear indication of the vibratory motion of the molecules. He gives as the result of his enquiry that electrical transmission is the result of a series of very rapid

* Comptes Rendus lxxxix., p. 76-80, 1879.

vibration of the more or less elastic matter which it traverses, and he points out certain analogies between electric motion and sonorous vibrations. To this idea Professors Ayrton and Perry were previously led* by consideration of the Viscosity of Dielectrics, and they suggested the hypothesis that the molecules of a conducting substance had very much more translational motion, and less rotational motion than the molecules of an insulator.

Professor Challis, of Cambridge, has extended this view so far as to embrace magnetism, electricity, light, heat, and gravity in one category of physical force, and to assert that they all result from motions and pressures of a uniform elastic fluid medium pervading all space not occupied by atoms. His views, however, have not received much attention, for they are not based on the foundation of any new facts, and they are utterly subversive of many cherished principles deeply rooted in the scientific mind. It is to be observed, however, that he regards electricity as a form of force.

Mr. Crookes in his recent beautiful experimental researches into molecular physics in high vacua has still more conclusively proved the connection that exists between electrical action and molecular motion. In fact, his experiments are so brilliant, his expositions so lucid, that one can fancy one sees with the eye of the body that peculiar play of the molecules which can be evident only to the eye of the mind. Not only has Mr. Crookes established as a physical fact the kinetic theory of gases, and the molecular constitution of matter, but he has indicated the existence of a fourth state of matter where the molecules fly about without mutual let or hindrance. He has also led us to doubt the truth of the generally received opinion that an electric current flows from the positive to the negative electrode. It would appear from his investigations that the reverse is the case. Be that as it may, he has added one story to the structure of the molecular theory of electricity.

The criterion of a good theory is, however, its power of pre-

* *Proc. Roy. Soc.*, pp. 7, 8, Nos. 18, 1878.

diction. A false theory has never led to prevision. Neither the corpuscular theory of light, nor the fluid theories of heat and electricity, ever led to the prediction of something of which eyes had not seen nor ears heard. The triumphs of prediction in astronomy, sound, light, and heat are innumerable. Faraday predicted the effect of induction in lowering the velocity of currents of electricity and the action of magnetism on a ray of light. Sir William Thomson predicted that a current in passing from a hot to a cold part of a copper bar would heat the point of contact, while in an iron bar it cools it. Peltier predicted the cooling effect of currents on the junctions of thermo-electric pairs.

But the true identity of these physical effects is conclusively shown by their quantitative character and by their adhesion to the law of the conservation of energy. Take the case of the electric light: the consumption of coal in a furnace generates steam, the steam works an engine, the engine rotates a coil of wire in a magnetic field, the motion of the coil in this field induces currents of electricity in the wire, these currents of electricity produce an arc, and thereby heat and light. The energy of the coal is transformed into heat and light through the intermediate agency of electricity. Is it possible to conceive that this intermediate agency is anything but a form of energy? Take the case of the Bell telephone: the energy of the voice produces the energy of sonorous vibration in the air, the vibrations of the air cause the vibrations of the iron disc, the vibrations of the disc vary the magnetism of the magnetic field, this produces currents of electricity in a small coil in this field which vary the magnetism of the distant magnet, which in its turn throws its disc armature into vibration, and thereby repeats at the distant station the sonorous vibrations of the air, and thus reproduces the energy of the voice. A tuning fork comes to rest sooner in front of a telephone than when it is allowed to vibrate freely in air. Here we have the energy of the fork passing through the several stages indicated above, and ultimately coming out in its original form. The energy of sonorous vibrations at the distant station is that lost by the vibrating tuning fork.

Is it possible to assume that in this cycle of changes energy has

been transformed into matter and matter again formed into energy? It is impossible and absurd. Clerk-Maxwell said—"When the appearance of one thing is strictly connected with the disappearance of another, so that the amount which exists of the one thing depends on and can be calculated from the amount of the other which has disappeared, we conclude that the one has been formed at the expense of the other, and that they are both forms of the same thing."

Would it be possible to light the streets of New York by the energy of the falling water at Niagara, as has been suggested by our Past President, Dr. Siemens, if the cycle of changes from the one spot to the other were not all different forms of this same energy? Would it be possible to plough a field a mile away from the source of motive power of the transmitting medium if the electric currents were not forms of the same power? Electricity in its effects is and must be a form of energy.

The final stage into which any physical theory grows is that in which every action can be expressed in mathematical language, where every phenomenon is calculated upon an absolute physical basis, and where we can foretell exactly what will occur under any possible emergency. This is the present condition of the science of electricity. We can calculate exactly how much steam power is required to generate a given current to produce a given light. We can tell precisely what dimensions of cable are necessary to give a certain number of words per minute on the other side of the globe. If a fault develop itself in a long cable through the gastronomic propensities of a thoughtless young teredo, we can calculate to within a few fathoms the locality of his edacious depredation.

Take, again, the conversion of heat into electricity, and of electricity into heat, of chemism into electricity, and of electricity into chemism—but I am tired of these illustrations. It may suit a Darwin or a Faraday, to satiate his pupils with a plethora of illustrations to carry his point, or it may suit an author who is writing a book to exhaust his subject, but the President of a Society in his inaugural address, must regard the time before him and the patience of his readers. I have shown that electricity is

not a form of matter, and I hope I have now convinced you that it must be a form of force. In its effects which are known to us, it must rank as one form of energy in the same category with chemism, with light, and with heat, as a peculiar mode of motion of the molecules of matter.

Clerk-Maxwell,* in his classical work on electricity, has used a somewhat curious argument to show that electricity is not, like heat, a form of energy. He says that energy is produced by the multiplication of "electricity" and "potential," and that it is impossible that electricity and energy should be quantities of the same category, for electricity is only one of the factors of energy, the other factor being "potential." But this does not militate in any way against the force of the argument, for in nature we can no more separate electricity and potential than we can separate heat and temperature. Energy usually appears as the product of two factors, and it is the equivalent of the work done. Thus, *Potential energy* is the product of mass and gravitation acting through a distance. *Kinetic energy* is the product of mass and the half-square of velocity. The energy of fluids is the product of volume and pressure. The energy of heat is made up of heat and temperature, and the energy of electricity is the product of electricity and potential. Hence it is that electricity, *per se*, may be said to be a form of force, while all its effects as known to us are forms of energy. Force alone cannot produce energy; it must be force and something else. Force is the power of producing energy, and it must have something on which to produce it. Hence matter is always present; and thus, though heat, light, and electricity are forms of motion, they are in reality properties of matter from which they are inseparable. They are evident to us through the play of the molecules of matter, and thus are properly called molecular forces.

Our Society was established for the general advancement of electrical and telegraphic science, and more particularly for facilitating the exchange of information and ideas among its members. The advancement of physical science is as much our duty as is the

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* Vol. i., p. 30.

furtherance of the art of telegraphy. Complaint has been made that we have devoted ourselves too much to the ventilation of purely concrete matters, while abstract questions have been neglected. There may be some truth in this stricture on our proceedings, and there may even be a cause for it. The Council are now seriously considering, not perhaps so much an enlargement of the scope of the Society, as an alteration in its title which will prevent any misconception as to its aims and purposes. Though the science of electricity is very advanced, there is still vast room for research, and our widely scattered members, spread all over the face of the globe, with the most costly and perfect apparatus at their command, have a grand opportunity to further the objects of the Society, if they only avail themselves of the means at their disposal. Every science, and especially our science, is the outcome of intelligent observation and careful experiment. There is no science that owes so much to its practical employment for useful purposes. The science itself has grown with the art of telegraphy. Practical difficulties have suggested enquiry, enquiry has prompted trial, trial has opened the eye of the mind to further development, and the result is that the highest scientific authorities acknowledge their indebtedness to the Telegraph Engineer. The historical inductive philosophy of Bacon is supposed to teach us that to establish science on a true basis, we must accumulate a vast array of facts, marshal them in proper methodical order, and extract from them laws. But this is not the natural order, nor is it the method pursued by Newton, Faraday, or any great master of science. Joule has spoken of "a new era in the history of science when the famous philosophical system of Bacon will be to a great extent superseded, and when, instead of arriving at discovery by induction from experiment, we shall obtain our largest accessories of new facts by reasoning deductively from fundamental principles."*

Faraday said:—"I must keep my researches really *experimental* and not let them deserve anywhere the character of *hypothetical imaginations*."† Nevertheless all Faraday's re-

* *Phil. Trans.*, 1858.

† Life of *Faraday*, vol. ii., p. 35.

searches are characterised by the hand following the brain; by observation and experiment checking, as it were, the speculations of thought. For forty years he pursued his pet notion of the connection between magnetism and light. He was sure it existed, but it took him twenty-three years to prove it.

Jevons said:—"Modern science is not the result of the Baconian philosophy, it is the result of the Newtonian philosophy and the Newton method. The 'Principia' is the true 'Novum Organum.' It is the result of theory guiding experiment and yet wholly relying on experiment for confirmation; the brain guiding the hands."* In our practice difficulties have suggested remedies, effects have suggested causes, hypothesis has suggested experiment, and experience has confirmed or refuted the hypothetical anticipations of the fertile brain. The difficulties in working underground wires, and in cables, suggested to Faraday and to Thomson those researches which have placed the laws of induction on "the solid ground of nature." The necessities of the Telegraph Engineer prompted the members of the Committee of the British Association to labour with such success as to bring the physical quantities of electrical elements within the fold of exact and absolute measurement. We glory in the present day in the perfection of apparatus which the requirements of telegraphy have evoked, and experiments which Newton would have thought too minute to fall under the observation of our senses are repeated every day. The indications of Thomson's reflecting galvanometer, and the beats of Bell's telephone are only equalled by Whitworth's gauges and by spectrum analysis.

The development of ocean cables has opened a new world to the scientific observer, and *Porcupines*, *Lightnings*, and *Challengers* have probed the deep unfathomed caves of ocean, swept the bed of the mighty deep, and scoured the surface of the seas, to search out new facts, and to open up fresh fields for the naturalist, the biologist, and the meteorologist. There are many questions that remain unanswered, and that offer work for our members: the distribution of atmospheric electricity, the conductivity of the atmosphere, the cause and nature of earth currents, the electrical

* Principles of Science, vol. ii, p.

relation that exists between the sun and the earth, the cause of electrification or polarization in insulated wires, the various phases of induction, and many other points deserve enquiry, for report to the Society for discussion at our meetings, and for record in our proceedings.

Earth currents have been a favourite subject of enquiry of mine for many years. I have always entertained the idea that they are directly due to the action of the sun. Some disturbance in the sun causes, by induction, a variation in the distribution of the lines of potential on the earth's surface, and produces the conditions required for these currents. I have many facts to support this hypothesis, but I want more to confirm it. I want observers to record the times of daily maxima and minima. I want them especially to note during those periods of unusual disturbance the direction of the circuits which are *not* affected, for they would give the direction of the lines of equipotential. This not only offers a useful field of observation, but its failure or success will illustrate the modern method of scientific research, when the brain suggests to the hand and the eye what they have to do, and what they have to look for.

And this idea which I have so long held on account of my observations of earth currents seems now in a fair way to be established, for one consequence of Professor Clerk-Maxwell's theory of electric action is, that a body with a statical charge of electricity when it moves rapidly, produces all the magnetic effects of a current. The experiments of Professor Rowland have shown that this result is correct, and Professors Ayrton and Perry, in a very ingenious mathematical investigation published by the Physical Society,* have proved that a spherical body in space charged with statical electricity and rotating on its axis, must behave like a permanent magnet, so that terrestrial magnetism must be due, in part, to the statical charge on our globe.

Professor Rowland has since shown that terrestrial magnetism cannot be *entirely* caused by the whirling of this static charge,

* *Proc. Phys. Soc.*, vol. iii., Part II., p. 57.

but he agrees with the calculations of Messrs. Ayrton and Perry, proving that a certain small magnetic force is actually produced on the earth's surface by this cause, independently of the existence of all other bodies.

Hence, anything which disturbs the distribution of the earth's charge, such as, for instance, the introduction between the sun and the earth of patches of different gaseous media (which we know from the previous experiments of these gentlemen affects the specific inductive capacity of the intervening space), or the alineation of some of the heavenly bodies with the earth, must affect the lines of static electric induction between the sun and our planet, and, therefore, affect terrestrial magnetism.

Our Society not only offers a useful field for scientific research, but it supplies a very useful arena for the education of its members. We shall shortly have open to their perusal the finest electrical library in the world—that of Ronalds, and we have the hopes in the future of having added to it an equally famous one, collected by one of our most eminent Past Presidents.

But knowledge gained from books should be confirmed by experience. We are too much apt to rely on illustrations, and think we know all because we can illustrate it by pen or pencil. Many a bright idea is lost for want of an opportunity to test its validity. Those who reason theoretically without demonstrating experimentally are sure to fall into error, while those who experiment in a haphazard way without being guided theoretically, are sure to waste time and money. I want, therefore, to see our library supplemented by a laboratory, where those of our members who have not the means at their disposal, can not only confirm their reading, but go and examine their ideas for themselves by the rigorous test of experiment. We are not all Hughes who can make scientific apparatus out of lucifer boxes and bonnet wire. If the financial abilities of our Hon. Treasurer prove as successful in the future as they have done in the short career which has distinguished his past occupation of the office, I have little doubt that this wish will be within reach of accomplishment.

There is no more useful mode of imparting knowledge than

that of lecturing. "It blesseth him that gives and him that takes." The lecturer often learns more than his audience. The possession of a laboratory would enable us to ask those who have made any special line of study their own, to favour us occasionally with a *vivâ voce* explanation of their researches and their views. Several societies practise this salutary plan, which is one that was very strongly advocated by Faraday.

Our Journal forms a most useful vehicle for the promulgation of new facts. History tells us of many brilliant experiments and discoveries that have been hidden and neglected from the want of proper publication, or of some society to take them up. Thus Mayow, a doctor of Bath, discovered oxygen over one hundred years before it was rediscovered by Priestley and Scheele. Romagnosi, a doctor of Trent, discovered the deflection of the needle fifteen years before Oersted startled all Europe with its rediscovery.

The great cry of to-day is for technical education. Here we have an organisation well suited for furthering this object, and with a home of our own, an unequalled library, and a well equipped laboratory, we ought to be able to stifle the cry, at least as far as telegraphy is concerned.

The science of electricity owes its great development to the vast extension of submarine telegraphy throughout the oceans of the world. The Atlantic has been crossed by no less than nine cables, of which three, however, are dumb. A thin cord duplicated the whole way, extends from England to Australia, it is duplexed to India, and one wire extends from Aden to the Cape. There are no less than 97,568 miles of submarine cable now in working order. In spite of the wail of distress that has depressed the commercial world, cable enterprise has not fallen below the average during the past year.

During the past twelve months the Telegraph Construction Company has manufactured 6,800 knots, Siemens Brothers 3,600 knots, the Silvertown Company 900 knots, and Mr. Henley 107 knots of cable—11,407 knots of cable laid by commercial enterprize in all quarters of the globe in one year!

The maintenance of the long mileage of cable requires the

employment of a fleet of ships. There are now no less than 27 steamships employed for telegraphic purposes in different parts of the globe. They are all well officered and specially fitted up with machinery and apparatus to facilitate their operations. They are each provided with a skilled staff specially trained to observation and experiment. We are much indebted to Mr. Jamieson for a very interesting paper on Cable Grappling and Lifting, but with this exception we have little or nothing to chronicle of their doings. It is surprising how few are the facts that have been added to our knowledge by such an organisation. It is owing certainly not to want of zeal, but perhaps to an ignorance of what is needed. If they were to study Sir Wyville Thomson's "Depths of the Sea" or his "Voyage of the Challenger," if they were to provide themselves with a good microscope with sample bottles and alcohol, they would be surprised to find how much they would increase their own enjoyment, afford gratification to their friends at home, and further the interests of science. Indeed, ocean life is becoming a source of considerable anxiety to the Telegraph Engineer, for many little creatures have developed a decided *penchant* for the constituents of which cables are composed. In all the recent cables laid within depths of 100 fathoms by the Telegraph Construction Company a wrapping of thin brass has been externally applied to provide for this growing evil. Damage to the gutta percha has not been observed at greater depths than 100 fathoms, but some specimens of the Atlantic Cable of 1865 which have recently been recovered from a depth of 2,000 fathoms, gave unmistakable evidence of the destruction of the hemp by some boring animal.

At home, the greatest advances have been made in apparatus. The mileage of wire that has been erected in England to meet the enormous increase of telegraph business that has occurred since 1871 is trifling. The average weekly number of messages in the month of October, for the former year, was 273,000, the greatest number ever reached at that time, while for this last year it amounted to no less a figure than 586,000. This great increase has really been provided for by the improvement in Wheatstone's Automatic Apparatus; by the introduction of duplex working,

and more recently by the quadruplex system. The duplex and quadruplex systems have been proved to be applicable to automatic working, and the speed of working long circuits has been much increased by the insertion of intermediate relay stations. But we have reached a limit; our system is actually gorged with messages; failures of wires cause considerable trouble, inconvenience, and complaint, the public—our watchful but not lenient masters—are beginning to growl, and the erection of additional wires has become essential. It is hoped that a new trunk line to the North and many additional wires will be erected during the ensuing summer. There is also strong reason to believe, that for the first time since the acquisition of the Telegraphs, the balance on the right side will be sufficient to show a surplus over the dividend on the capital expended in the acquisition of telegraphs by the State.

The improvement of apparatus in this country is a veritable case of growth by evolution. The instruments themselves are in a constant state of transition. Every defect or deficiency as it develops itself is removed, and the result is that the instrument grows by a process of careful cultivation and of intelligent selection, prompted by experiment, guided by theory, and confirmed by practice. It is not too much to say that in quality of manufacture, efficiency of performance, and perfection of design, the apparatus of this country takes its position in the very front rank. This applies not only to the apparatus used by the Post Office, but essentially to that employed by the Cable Telegraph Companies. Even lately a very great step forward has been made by Mr. Harwood, of the Eastern Telegraph Company, who has recently much improved the duplex working of cables, both as regards speed and efficiency.

The cry that invention has been checked by the monopoly of the Government is made by those who are ignorant of contemporaneous history, who are too callous to enquire for themselves into the truth of the accusation, and with whom most probably the wish is father to the thought.

The telephone—the great excitement of the previous session—has made but little progress during the past year, though there is no doubt that, through the discovery of the principle of the micro-

phone by Professor Hughes, the practical applications of that wonderful instrument have been much improved. In fact, Professor Graham Bell, Mr. Elisha Gray, and most of those who have been working in this field, have laid aside their own particular form of transmitter, and have adopted one that is a mere form of microphone. In England we have one or two excellent forms. Those of Mr. Louis Crossley and Mr. Hunnings leave little to be desired, while in America that of Mr. Francis Blake is admirable, and is also much used in England. Litigation has commenced between the Post Office and the telephone Companies, not to restrict, or in any way to interfere with the use of the telephone, but to prevent the establishment of a particular branch of Post Office telegraph business without its license or consent.

A curious controversy as to the seat of the vibrations, which result in the reproduction of speech, has occupied the attention of certain physicists on the other side of the Channel. Some following Graham Bell and led by Count du Moncel attribute the effect solely to what may be called the Page effect, viz., a molecular disturbance of the magnet itself. Others led by Colonel Navez prefer to follow me in a theory I first propounded at Plymouth,* viz., the sonorous vibrations are set up by the mechanical movement of the disc itself. Both theories are really true, and the controversy is, like many other scientific controversies, a war of words. The motion of a mass is the integration of the motions of the molecules of the mass. There is the Page effect in the magnet, and there are the effects of magnetic attraction on the disc; the resultant action being sonorous vibrations. In point of fact, there would be no sonorous vibration if there were no molar motion, and there would be no molar motion if there were no molecular disturbance.

One of the most "beautiful developments" of the year is the writing telegraph of Mr. Cowper. While the telephone reproduces speech, here we have the handwriting reproduced by an operation that is almost magical in its character.

The electric light has been making considerable progress, and

* British Association Meeting, 1877.

is gradually forcing itself into practical use, in spite of many of the drawbacks to its employment that have yet to be removed. The lamp of the future has not yet been produced, though steadiness and duration have very much advanced during the past twelve months. There is very little room for improvement in the generating machine, for both the Siemens and Gramme machines convert about 90 per cent. of the energy thrown into them into electric currents, and this is a duty which no other kind of machine can show.

One of its most notable and useful applications has been on board ship, to further the operations during the night in laying and repairing cables. I was present on board the s.s. *Dacia*, in the Mediterranean, when this was done, and the success was unequivocal.

The Brush machine has recently been introduced into this country, and its performances are certainly very wonderful. It produces an electromotive force of over 800 volts, and I have seen it maintain 20 very steady arcs joined in series. Sixteen appear to be its efficient limit, and this number of lamps, giving over 1,000 candle power, are easily maintained by an expenditure of 13½ horse power. The performances of the Brush light are certainly the most advanced form the electric light has yet taken. There are over eight hundred of these lights in the United States; and it is worthy of notice that it has quietly crept into existence without the aid of the ubiquitous and omniscient newspaper correspondent, or the transmission of any sensational telegrams, to the detriment and discomfort of gas shareholders.

It is assumed by many that the electric light is devoid of heat, but Professor Dewar has shown that a Siemens arc radiates heat equivalent to 3 horse-power per minute. Moreover, the use of such powerful currents, unless carefully directed, are dangerous to life and limb, and may even, unless properly protected, result in fire.

Gas is not going to be affected by the electric light. The proper function of gas is to generate heat. Ninety-four per cent. of the ingredients of gas are consumed in generating heat, and only 6 per cent. in producing light. It is remarkable that so amenable and tractable an agent for heating purposes has not

been more utilized, but the fact is that the public is ignorant of its properties, careless of its employment, and callous of its defects. It is not too much to say that 50 per cent. of the gas manufactured is absolutely wasted for illuminating purposes by the wild extravagance with which it is burnt, and by the want of those systems of regulation which have been introduced to compensate for irregularities and excesses of pressure.

The utilization of the illimitable wasted energy on the earth's surface offers a fine field for the ingenuity of the Electrician. The tides of the ocean, the motion of the atmosphere, the rapids of a river, the innumerable waterfalls that are found in every mountainous or hilly country could be compelled to give up in the form of electric currents that energy which gives them existence, and which could thus be employed for providing power, generating heat, or supplying light, away altogether from their source of conversion. In fact, Sir William Armstrong, near Newcastle, and an enterprising hotel proprietor in Switzerland have already produced light in their houses by converting into that form the energy of a neighbouring waterfall.

I cannot omit mentioning the researches of Professor Hughes. Here we have the case of a telegraphic engineer who has spent the best part of his life in improving the working of telegraphs, resting from his labours with well lined purse, and turning philosopher. Spurning the costly apparatus of the philosophical instrument maker, he takes the simplest and cheapest materials he can lay his hands on, and with fine inventive skill, aided by an instinctive mechanical turn of mind and well trained hand, he forces nature to develop unexpected secrets, and he enables the chemist of the mint to detect an infinitesimal trace of impurity in the bright golden sovereign. Moreover, he has not only detected an absolute zero in nature, but he has given the physician a standard by which he can measure the relative sensitivity of the dullest and sharpest ears. His induction balance and audiometer are fit sequels to his microphone, and I venture to predict that he will yet wring out of Dame Nature some other facts that have lain dormant since the first fiat went forth, "Let there be light." His mode of investigation is a very apt illustration of the practice I have

previously described as so successful in modern scientific discovery, of making experiment subservient to hypothesis with the view of furthering science. His investigations have been made without the slightest idea of gaining "filthy lucre" or of taking by a flank movement some successful inventor's patent—practices which, I am sorry to say, in these immoral times are far too frequent to be palatable.

We cannot but congratulate ourselves upon the position of our Society. Our first President, Dr. Siemens, in the first paragraph of his inaugural address, remarked that, "Some years must necessarily elapse before our Society can have given substantial proof of its useful action." I venture to submit that those years have passed. Our success has exceeded the most sanguine anticipations. Our work has been thought of sufficient importance to merit special reference in the first annual address of the gifted and able President of the Royal Society. The good that our Society has done and is doing is patent to all. It stimulates thought, it encourages experience, it furthers the science, and it developes the art of telegraphy. It inculcates knowledge, and is therefore educational; it records results, and is therefore historical; it cements friendship and promotes good fellowship, and it has therefore a moral influence; but, above all, it knits together in one family bond that great wide-spread brotherhood who are found scattered in every quarter of the world, maintaining and operating "The wonder-working wire," to the annihilation of space and the economy of time.

At the conclusion of the address—

Mr. Latimer Clark rose and said: Gentlemen,—I am sure it will not be a mere matter of form with us, but a matter of real feeling, if we return our heartfelt thanks to our new President for the very able, and, of course, very eloquent address that we have just listened to. I will not attempt to eulogise it, but content myself with simply proposing that the best thanks of this Society are due to Mr. Preece for the able address he has delivered, and that, with his permission, the same be printed and put in the Journal of this Society.

Mr. SPAGNOLETTI had much pleasure in seconding the proposal, and remarked that such an instructive and philosophical address deserved an exceedingly hearty vote of thanks.

Mr. PREECE: I can only say, Gentlemen, that I am highly delighted to think that this evening is over, but am ten thousand times more delighted to find that my efforts have met with your approval. I can only add that as far as this year of office is concerned, I shall do all I can to merit your approval, and to further, to the best of my ability, those objects of the Society which I endeavoured to sketch in the latter part of my address.

The following new Members and Associates were then elected :—

*Members:*

Sir James Anderson.
Henrique Snell.

*Associates:*

William Fereday Bottomley.
J. E. Greenhill.
Richard Hogge.
Joseph Ismay.

and the meeting declared adjourned till Wednesday, 11th February, 1880.

The Eighty-Third Ordinary General Meeting of the Society was held on Wednesday evening, February 11, 1880, at the Institution of Civil Engineers, 25, Great George Street, Westminster, Mr. W. H. PREECE (President) in the Chair.

The usual preliminary business having been gone through, the SECRETARY read the following Paper on

## TELEGRAPHY IN NEW ZEALAND.

By T. A. MAGINNITY, Member,
*Assistant Secretary for New Zealand Telegraphs.*

Some five years ago, and before I had the honour of becoming a Member of the Society of Telegraph Engineers, I wrote a short paper bearing the above title, which appeared in the *Home News.* Since reading the paper by Mr. Preece on the American Telegraph System, I thought it would not be uninteresting to members of the Society to become acquainted with some of the details of our Colonial Telegraph System; I have therefore made use of some of the information contained in my former paper, so far as it would suit the purposes of this one, and the information I have now compiled I place at the disposal of the Society. There are, of course, many points in connection with our general system respecting which I could have gone into further detail, but I have endeavoured to afford information concerning the system as a whole without wearying my readers, and without extending my paper to an undue length.

The first attempt to introduce the system of telegraphy into New Zealand was made by the Provincial Government of Canterbury in the year 1862, the line first erected being from Christchurch to the seaport town, Lyttelton, a distance of eight miles.

The example was quickly followed by the sister province of Otago in the early part of the following year, the work being the placing of Invercargill and Bluff in telegraphic communication with each other.

The next line erected was from Dunedin to the seaport town, Port Chalmers, a distance of 7 miles. This was completed in the latter part of 1863.

The work to connect Dunedin, province of Otago, and Christchurch, province of Canterbury, a distance of 255 miles, was commenced on the 4th October, 1864, by the General Government. This line was completed, and the first message from Christchurch to Dunedin transmitted on the 13th April, 1865. The line was carried north to Nelson, embracing the intermediate Gold Fields Settlements, and completed to that town in April, 1866.

The laying of the Cook Strait Cable connecting the North and South Islands, or correctly speaking, the North and Middle Islands, was successfully accomplished in August, 1866. Up to the early part of the same year, no attempt had been made to establish telegraphic communication in the North Island, with the exception of a few military stations opened by the military authorities in the Waikato Country, during the Waikato War of 1862-63. After the laying of the cable, however, communication was quickly established with Auckland, on the east side, taking in all the intermediate stations of any importance, including Hull, Featherston, Greytown, Carterton, Masterton, Napier, Taupo, Tauranga, Grahamstown, &c., and to Taranaki (New Plymouth) on the west side, embracing Foxton, Palmerston, Wanganui, Patea, &c., &c.

The line from Napier northwards was erected under very great disadvantages, and the work was accompanied with considerable danger, owing to the hostile nature of the native tribes then residing in that part of the country. This state of things is now happily changed, and no difficulty is experienced in carrying out any desired works, the country being in a thorough state of peace and civilization; to such an extent, in fact, have the natives become loyal and civilized, that they hail with pleasure the arrival of travellers and tourists, to what was for many years a perfect *terra incognita* so far as Europeans were concerned, viz., the beautiful lakes and terraces of Rotomahana so graphically described by Domett, Tinney, Trollope, and by other writers. The line extending from Tarawera (a small station 50 miles north of Napier) to Tauranga, a distance of 160 miles, passes through a country, the scenery of which may perhaps be equalled, but certainly not surpassed by any in the world. It is here the great mountain of native tradition, Tongariro, an extinct volcano, arises. Here also

are the lakes and mineral springs, the healing nature of which are now only becoming known, and are beginning to be appreciated. This I venture to assert, that in years to come this part of the colony will become one of the greatest places of resort in the Southern Hemisphere. To give this part of the North Island anything approaching a fair description, would take time and space which I in this paper cannot attempt, even were I bold enough to think that I could treat the subject with ordinary ability. To note in detail the progress of the various lines and works throughout the colony, would probably not prove sufficiently interesting, so I will at once proceed to jot down in a condensed form, a general summary of our system as a whole, and hope that my efforts to prove that New Zealand is not the unimportant or uncivilized country some of our friends at home think it, will not be altogether unsuccessful. If my paper proves of the slightest interest to members of the Society, and finds a place in the Journal, I shall be more than repaid for any trouble it may have cost me. I may state that the whole of our telegraph system is now under the control of the General Government, the provincial and military lines already referred to having been taken over at a valuation some years ago.

On the 30th June, 1866, the number of miles of line erected was 699, and the number of stations opened for business 13; since that time to the present date our works have been speedily extended, till our system reaches from Mongonui, almost the extreme north of the North Island to Bluff, the extreme south of the South Island, affording telegraphic communication to all cities, villages, and townships of any importance intervening. We have now in circuit 3,543 miles of line, carrying 8,444 miles of wire, which is exclusive of works in progress. These latter, when completed, will increase our mileage of line to 3,621, and wire to 8,868 miles.

During the year ended the 30th June last there were transmitted 1,448,943 messages of all codes, being an increase of nearly 14 per cent. over the previous year. This was effected at a cost of £96,801 8s. 3d., but taking credit for the value of Government messages transmitted we show a credit balance sufficient to pay 3½ per cent. on the capital invested (£415,641 14s. 8d). The

number of stations now open to the public is 195, and the number of officers, including linemen and inspectors, in the employ of the department is 801. This for a colony numbering less than 415,600 inhabitants, will, I am sure, be admitted as very satisfactory and progressive, taking into consideration that the colony is only some forty years old.

On reading Mr. Preece's paper on the American system, I find there are many points in which our New Zealand system and that are similar. We have a system of "collect" message, according to which a telegram may be sent to be paid for upon delivery by the receiver. As, however, this system might be open to abuse by persons sending telegrams of a frivolous or useless nature to some friend, perhaps, by way of a joke, it was necessary that the department should be in some way protected, and, therefore, the law concerning "collect" messages is, that if the receiver refuses to pay, the sender becomes liable, and from him the fee due can, in the event of his also refusing to pay, be at once recovered by recourse to law.

In our tariff on ordinary messages, also, I find we agree in the system of charging, viz., at per word after the first 10 words. Our rate is one shilling for the first ten words and one penny per word after, address and signature up to ten words being allowed free.

By a return recently prepared for Parliament I find that, as regards the press tariff, our newspapers are dealt more liberally with than in any other country where the system is in vogue. Our press rate, between the hours of 8 a.m. and 5 p.m., is 6d. for the first 10 words and ½d. for each additional word. To evening papers, however—that is, papers publishing before 5 p.m.—a further concession is granted by allowing them 1,500 words during the day at evening rates, which means the rates after 5 p.m., viz., 6d. for the first 25 words, and 3d. for every additional 25 words or fraction thereof. I am sorry to have to state that even all this does not nearly satisfy the rapacity of our literary friends, so, in addition to all these considerations we have to man two special wires extending from Auckland to the Bluff, taking in all the chief centres of population—every evening from 8 p.m. till 1 a.m. next morning, for which we get a stated subsidy.

Again, we have what is called in America the "deferred" system, but what we call the "delayed," by which a telegram is accepted at any station at any time during the day, and posted at its destination the same night, being passed along the wires at the convenience of the department. For messages of this code we charge one-half the ordinary rate, in addition to a fee of 1d. for postage. For the year just expired 56,721 messages of this code were transmitted, which speaks well for its popularity. Then we have an "urgent" code, which takes precedence of all other codes. For messages of this class we charge double rates, and I see by our last returns we transmitted for the year 30,106 U. O. T.'s.

Again, we have a money order system similar to that in vogue in America. By this system, sums of money (not exceeding £10 in any one order) may be sent from any money order office to any other money order office with which it is in telegraphic communication. The charge made by the Post office is 4d. in the pound commission, in addition to the telegraph fee of 1s. The amount of money transmitted by this means during the last year was no less a sum than £61,693 9s., effected by 14,607 orders.

A repetition of any message may be obtained by paying a half rate additional, and instructing the clerk at the time of presentation that a repetition is desired. "Reply paid messages" are also accepted for transmission, the sender of the original giving instructions as to the delivery of the reply on arrival, the fees due upon which are collected when delivered.

We have a system of shipping reporting in force, which enables the whereabouts of any steamer to be known. The arrival and departure of every steamer at or from any port in the colony is sent to Wellington, and posted up on a board kept for the purpose in the Government offices. In addition to this, a shipping board is posted outside of every sea-port station, giving, for the information of the public, the arrivals of steamers which have sailed from that port, and the departures of steamers for that port; thus, at Wellington would be posted the arrival of a steamer at Lyttelton, which had probably left Wellington the previous day, as also would the departure of a steamer from Nelson, bound for Wellington. We have also a complete system of wind and weather signals, sent

to the Government Buildings at Wellington daily, and posted up on a properly arranged board. This information, with other observations, is also furnished to the head weather reporting office at Wellington, from which is compiled further information for sending out forecasts.

Chambers of Commerce are entitled to receive all shipping information at the rate of 3d. per message, and the captains of vessels can telegraph to harbour masters at any port to which they may be bound to ascertain state of bar, wind, or tide, for 6d. per message, which charge also covers the reply.

Our free delivery extends for a radius of one mile from the receiving station, and for telegrams beyond the mile a small charge for porterage is made, according to the distance.

At every station a cipher register is kept where any person may register their full address in cipher, no charge being made for registration. This facility is largely taken advantage of by mercantile people and others having telegraphic communication with England, or the Australian Colonies, where the saving of a word is a consideration.

Our present staff of operators are nearly all young men, who have been trained in the colonies, and are, without exception, "sound" readers. The salaries of operators range from £120 to £200, and those of officers in charge of first and second class stations, from £200 to £375 per annum.

Our New Zealand youth displays great liking for telegraphy, and shows considerable aptitude in gaining a knowledge of operating. Cadets enter at ages ranging from sixteen to twenty-one, and undergo a course of three months training in the "Learners' Gallery" under an expert operator. Here they are instructed also in connections and manipulation of the test board. At the end of three months, when they are drafted out to stations, they are competent to take a fourth class circuit, being able to send about 25 words, and receive by sound from 18 to 25 words per minute. They are trained in batches of 24, in a room fitted up for the purpose. The instrument in general use is the Morse, but the small and convenient sounder is now becoming general at all the principal stations.

Our lines throughout the Colony are of the most substantial construction, the wire used being No. 8 Galvanized B B B, carried on poles varying in size from 20 to 30 feet (as the nature of the country demands), sawn from the heart of the Totara (*Podocarpus*). About 20 poles are inserted to the mile, excepting in main thoroughfares, where the number is increased, and poles of greater length used.

The joint used in attaching wires is the Britannia joint, and every lineman is bound by his regulations to solder every joint made by him, failure to do which is, on discovery, visited by severe fine or probable dismissal. The result of these precautions, is that our lines are maintained in first-class electrical condition, as regards insulation, &c.

The poles are sawn square from the heart of the tree, being for 20 and 25 feet poles $8'' \times 8''$ at the butt, tapering to $6'' \times 6''$ at the top. The 30 feet and 35 feet poles are of course increased in proportion to their length.

The ordinary arm used is sawn from the heart of the Rata tree (*Metrosideros Robusta*). It is 27 inches long, 2½ square, and bored in the centre for bolt, and at either end for insulator. It is very durable, and bears an enormous strain. The insulator which has for years past been in use is the Varley (brown earthenware), but this is being now quickly superseded by white Prussian porcelain, which is looked upon as being both more efficient and of greater strength. Its appearance is also strongly in its favour. The system of earth wires was at one time largely in use, but they proved to be exceedingly troublesome by causing leakage, and are now only used in very exposed places where lightning is prevalent.

The leading-in wires from the main lines to offices consist as a rule of two strands of No. 16 binding wire twisted, forming one conductor, but from the test board to the instrument table and instrument, as well as all external connections, the covered wire G. P. taped and tarred is used.

The battery in general use is the Daniell cell, with copper and zinc element and gutta percha tube. To maintain this battery costs about 1s. 1½d. per cell per annum. Fuller's bichromate battery has been introduced on some of our longer circuits, and has proved very efficient, one cell being equal to three of the Daniell's.

The condition of every section of line is telegraphed to the General Manager at 9 o'clock every morning, and entered by him in a diary. Records of tests made by the various inspectors of the sections under their respective controls, are also forwarded every month, and after examination by the General Manager are filed in guard books kept for the purpose. Besides these records of tests, each inspector renders to the head office, at the beginning of every month, a report of the section or sections of line in his district, setting forth faults which have occurred and the origin of the same, also all, if any, repairs effected during the month previous. There are at present in the employ of the department for maintenance purposes five inspectors, two sub-inspectors, and fifty linemen. Linemen are placed at intervals varying from 20 to 40 miles, and when a fault occurs on a section the linemen from both ends start and travel till they meet.

The movements of every lineman are reported to the General Manager at head office as well as to the inspector of his district.

It is a rare occurrence for an interruption of any sort to exist longer than an hour, unless when by bad weather or swollen rivers the linemen are unable to reach the fault.

At certain points where the opening of a station was desired by the settlers, but the probable business not promising to be nearly remunerative, and yet where the department could, with advantage, utilize a lineman, it has been found extremely convenient to open what we call a "lineman's station," placing there a lineman with a fair knowledge of operating. While not absent on line duty his office is, of course, open to the public, but when called away for repairs, or any work connected with the line, he locks up his office, posting a notice of his absence on line. This arrangement, while meeting the convenience of the settlers, proves advantageous to the department. In carrying our lines through native country considerable difficulty is at times experienced through the stubborn opposition of the natives. This, however, is generally successfully overcome by good tact and judgment on the part of the officer in charge of the works, who must, if he wishes to carry his undertaking through, exercise the greatest patience, and under no circumstances lose his temper. The Maories residing on the

land through which the line passes invariably make a claim for compensation to some absurd amount, but by "chaff" (colonial phrase) and good humour, the claims become eventually modified to a stick of tobacco, or something of equal value.

The opposition, however, often causes inconvenience and delay in the progress of the work, more especially when the softer sex take up the imaginary grievance. Time to them is of no consideration, and on one occasion an old Maori woman squatted herself in a post hole, and held the position for hours, to the dismay and disgust of the construction party. To overcome native opposition, it is found a good stroke of policy to press them into the service by giving them small contracts for bush clearing, wire laying, and so on, and in their anxiety to see their respective contracts brought to an early termination, their former opposition is entirely lost sight of. With judicious management they can be invariably brought over to the right side, but the utmost patience, combined with firmness, is necessary to ensure success.

Our system of working here is the open circuit, and every station is fitted with a battery cupboard, about 7 feet by 3 feet, with shelves containing the line and local batteries, the former consisting of from 40 to 60 cells, according to the length of circuit, and the latter of 10 cells.

The duplex upon Dr. Lemon's system is now worked upon the No. 1 cable (42 miles), No. 4 cable (42 miles), Wellington to Napier (221 miles), Blenheim to Christchurch (206 miles), Blenheim to Dunedin (461 miles), Christchurch to Dunedin (255 miles), Dunedin to Invercargill (134 miles), and Napier to Auckland (346 miles). Experiments upon the quadruplex have been made with every degree of success, and this system will be introduced for practical working as soon as certain instruments now ordered arrive.

The Morse sounder is the instrument in general use, but the simpler form of sounder is now being largely availed of at all the principal stations.

We have at present two cables connecting the North and South Islands—the No. 1, containing three conductors, and the No. 2 (which was laid in March, 1877), 1 conductor. The former, which

was laid in 1866, was broken in December, 1875, and was repaired in January of the following year. . This is the only total interruption which has occurred since its being laid. The No 3 conductor within the No. 1 cable has now become entirely interrupted, and steps are being taken towards obtaining a second alternate cable, as it is looked upon as being extremely probable that the other two conductors will give out at an early date. From a series ot tests taken, it is proved beyond a doubt that the interruption tc the No. 3 conductor is at the precise spot where the old break occurred, and where the cable was spliced. The cables run from Lyell's Bay on the north, to White's Bay on the south side, and at either end are cable stations, properly fitted with lightning guards and testing apparatus. The cables are tested not less than once every month, this duty being performed by the General Manager himself in the early morning, prior to the opening of stations. All cable work is taken off at the Blenheim Station, and re-transmitted. For many years the transmitting staff was kept at the White's Bay Station, but it was deemed more convenient to transfer it to Blenheim, some 7 miles distant, placing a lineman in charge of White's Bay, to earth the cables in the event of lightning. The Lyell's Bay Station is only 3 miles distant from the chief station at Wellington.

The railways have now made such headway in this colony, that in the South Island a separate telegraph service for railway purposes, entirely distinct from our general system, is being organised. The charge of this service has been entrusted to an officer, Mr. Floyd, for some years connected with this department, and who had some considerable experience in railway telegraphy in England. Having only just entered upon the duties, he has of course not yet made much progress, but in the course of twelve months, I anticipate he will have his arrangements nearly completed. In the meantime, our wires are placed at their disposal for train signalling when necessary. It must not be understood from this that they are entirely without railway wires proper, as they have two wires extending from Christchurch to Dunedin, and one from Dunedin to Invercargill, but they have not yet got their own complete staff of operators, linemen, repairers, &c., and the wires

they are now using are carried on the poles bearing our main wires. In the North Island the railway telegraph system is carried on the lines and worked by the officers of this department, and will continue to be so worked until the further extension of railways and frequency of trains render an independent system necessary.

The head office of our department is at Wellington, the capital town of the colony, our administrative head being the minister holding the portfolio of Telegraph Commissioner. Our executive chief is the general manager, Dr. Chas Lemon, whose name no doubt will be familiar to many Members of the Society of Telegraph Engineers.

The introduction of ladies as operators was, in the first instance, purely experimental, and the result has not proved so successful as to render their further admission desirable. As operators, the per centage of females who become proficient is far less than that of the boys, and only in one instance has it occurred that a female has become a really first-class operator.

Our first-class operators send and receive on an average from 1,000 to 1,500 words per hour, second-class operators about 750 to 1,000 words per hour; this is the rate averaging their work for the day. The salary paid to cadets upon entry is £76 per annum, £80 in six months, £90 in twelve months, and £100 per annum in two years; their progress after that is entirely dependent on their own conduct and efficiency. The average quantity of press work sent from and received at our *central* station numbers from 75,000 to 80,000 words per day; this is independent of press matter exchanged between other stations and not sent to Wellington. During the week ended the 2nd August the number of telegrams sent from the chief stations was as follows: Wellington, 4,995; Dunedin, 2,166; Auckland, 2,199; and Christchurch, 1,695.

Our great anxiety is to keep the press off the wires as much as possible during the day, by offering special advantages and concessions after hours; but, unfortunately, while thus succeeding to meet the requirements of the morning papers we cannot get rid of the evening papers, and from what I have already stated with regard to the allowances made to evening papers it will be readily seen that the capacity of our wires is taxed to the utmost. The

result of this extreme pressure is that the operators are kept at their highest speed nearly all day long, and correctness is to a certain extent sacrificed to rapid transmission. Our errors, however, are not so very numerous, taking into consideration the quantity of work put through. Some of our chief sources of error are indistinct writing in the original message, use of figures, and the use of "coined" words for cipher words, which do not give a receiving operator the slightest chance to render them correctly. An endeavour has lately been made to get the banks to adopt a cipher consisting of simple words compiled from the latest standard dictionary, and I am in hopes that it will yet be adopted. I feel certain they will not regret the change.

All original messages, and the top copy of received messages, are, as I have already stated, forwarded to head office, and after being kept for two years they are destroyed. The copy of any message dated within two years may be obtained on application at head office, the charge for which is one shilling, in addition to a search fee of two shillings and sixpence per hour. This latter charge is necessary to secure the department from absolute loss, as it is found that in nearly 50 per cent. of the applications made for copies, the information given as to date, &c., is wrong, and therefore causes considerable loss of time to the officer whose duty it is to make these searches. The old forms are profitably disposed of to our colonial paper mills. The hours for which the principal and second class stations are opened to the public are from 8 a.m. till 8 p.m., and at other lesser important stations, from 9 a.m. to 5 p.m., Sundays and holidays excepted, when the hours are from 10 a.m. till 10.30 a.m., and from 5 p.m. till 5.30 p.m., double rates being charged.

Communication by telephone on private wires has within the last year or two come largely into use, and is adopted by many of our leading merchants, who have their chief places of business and detached warehouses connected. Siemens' dial instruments for private use are also becoming very popular, and the department has undertaken within late months, the erection of some twelve or fifteen private wires, varying in length from one to five miles.

Our system of fire alarm is not so perfect as that of our

American friends, but in our chief towns the various fire brigade stations and the principal Government buildings are connected. At Wellington there is a watch tower standing on a high and commanding position, which is in telegraphic communication with each fire brigade station, and also the police station, and on the first appearance of fire, the alarm and locality is immediately signalled to each station, where horses are kept in readiness day and night for immediate action.

The New Zealand station of the Eastern Extension Company is at a place called Wakapuaka, a picturesque inlet from Blind Bay, and about five miles north of the city of Nelson by water, fourteen miles by land. Wakapuaka is the landing place of the New Zealand and Australian cable, and here all messages are handed over to and by our department for further transmission. The staff of the cable authorities consists of a superintendent and three assistants, while that of this department consists of an officer in charge, and one first class operator. The two administrations, although under one roof, are entirely separate and distinct. The instruments used on the cable are the mirror and the siphon recorder.

As the English telegrams generally reach Sydney after midnight, a special service is now in vogue whereby these telegrams may be sent on, without remaining till the ordinary hours next day.

The cable operator has an alarm clock set to give the alarm at a certain hour.

On the arrival of the messages from Sydney he rings up our operator, who in his turn rings up the night operator at Blenheim. Blenheim then rings up Christchurch, on the south circuit, and Wellington on the north, who then ring up Dunedin and intervening special stations, and Napier and Auckland respectively. At all these special stations, the bell is put in circuit by the night operator, who sleeps within hearing of it. The work sent through is only press work for the morning papers, and does not generally occupy stations longer than a half hour in transmission.

I have now touched on nearly all the main points in connection with the general working of our colonial system, and, in conclusion,

will only express the hope that I have succeeded in affording some little interesting information respecting one of our New Zealand institutions, and of bringing our little Colony prominently before our friends in England.

The PRESIDENT: Mr. Maginnity's paper illustrates one of the great uses of this Society, viz., that by which we attract information, opinions, and discussions from all parts of the world. He speaks of the employment of females not being satisfactory. This is not so in England, where female operators are employed to a very large extent, and where it is found that their nimble fingers and rapid writing are most useful. Not only in telegraphy is this the case, but railway companies and insurance offices find it advantageous to employ females, who are patient, steady, attentive, and sit by their instrument or desk all day long (filling up spare moments with their woolwork), while the boy clerk would be impatient and eager for outdoor amusement. As regards the wonderful facilities offered in New Zealand to the Press, I would point out that while there they allow 25 words to go for sixpence, after five o'clock, in England we are compelled to accept one hundred words for twopence. This low rate causes the amount of news handed in to be overwhelming It is worth noting that nearly a million words of news are delivered every day in towns in the United Kingdom. Every town with a daily paper receives its portion of news direct from London, either parliamentary debate, election news, or general items of interest, and few people think of the wonderful machine by which all this is brought about at the exceedingly low rate I have mentioned.

As another instance of the utility of our Society, I may mention that the idea of utilizing old rails for telegraph posts having been described in this room in a paper communicated by Mr. Davids, Associate (a Telegraphic Engineer resident in Brazil), who found that such material, when available, was very useful for the purpose, was published in our Journal and read in New Zealand. There, as I learn by letter, the idea was grasped and acted on, thereby one of

the prime objects of the Society in disseminating useful information being carried out.

Mr. A. J. S. ADAMS supported that part of Mr. Maginnity's paper referring to obstructiveness, and the difficulties raised on the part of the natives to the carrying out of telegraphs. When lines were being erected in Persia he had found that Mohammedans would not work with tools which were made up of any part of animals "unclean" according to their faith, and that upon one occasion the workmen threw down the paint brushes they were using upon discovering that they were made of pig bristles. As neither coaxing nor threats overcame the difficulty, it became necessary to employ Armenians, who had no such religious scruples. Upon another occasion the erection of a local wire at Tabreez had to be abandoned after several attempts, because the natives so strongly objected to it, and pulled the wire down as fast as it was put up.

Mr. A. SIEMENS, following the same point as the previous speaker, could also testify as to the difficulties met with in foreign parts. When he was engaged on the line of the Indo-European Company between Djulfa and Tabreez he found it necessary to intimidate the natives, who rather fancied the wire for various purposes. He was able to do this in a very effectual manner, for having found that at that time of the year there was a thunder-storm nearly every afternoon, during which the line, being insulated, was charged by induction, he brought about a gathering of the natives and persuaded one of their notables to ascend a ladder and touch the wire, saying the wire would defend itself. On doing so, the man received such a shock that he fell down the ladder, and the wire was considered after that by the natives as being bewitched.

A vote of thanks was then accorded to Mr. Maginnity for his paper.

The PRESIDENT read the following :—

## ON THE DURABILITY OF SOME IRON WIRE.

By WILLIAM HENRY PREECE (President).

In the interesting electrical researches which M. Gaston Planté is making with his "rheostatic machine"*—a machine by which the charge of 80 condensers can be discharged either in "series" or in "multiple arc," and thus very powerful effects of "intensity" and "quantity" be produced—he has shown that the discharge of great quantities of electricity through fine wires renders them very brittle. Peltier and other observers have remarked on this effect of currents, but the practical experience of Telegraph Engineers has scarcely borne out this observation, probably because the ordinary currents employed in telegraphy are too feeble to produce any evident effects.

Lightning conductors, however, according to M. Planté, ought to give some evidence of this action, for they at times convey very powerful charges of electricity, and upon the authority of M. Calland, he asserts that lightning rods are frequently found very brittle. He attributes to this cause many accidents to such conductors apparently in perfect order. The observations apply equally to iron as to copper.

Though the effects on telegraph wires may be unobservable on account of the minuteness of the currents used, nevertheless long continued application might result in a different conclusion.

Four wires were erected on the London and South Western Railway, between Nine Elms and Gosport in the year 1844. They have thus been in incessant use for 36 years. At my request, Mr. Goldstone (member) cut out a span of this old wire between Bishopstoke and Botley, in Hampshire. "It seemed to me," says Mr. Goldstone, "hardly so good as some that might be looked out, but it was in fair condition." Unfortunately, we have no recorded specification of this wire, but we know that it was No. 8 B.W.G., and of the very best charcoal wire that could be procured and

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* *Comptes Rendus*, vol. lxxxix, 1879.

galvanized. Nor were electrical measurements known in those days, so that we have no knowledge of its original resistance. But the *present* dimensions, resistance, and mechanical strength may help us "to point a moral."

| | | | |
|---|---|---|---|
| The length of the wire is ... | ... | 69·333 yards. | |
| The diameter " " ... | ... | ·175 inch. | |
| The weight " " ... | ... | 16·268 lbs. | |
| " " " ... | = | 412·96 lbs. per mile. | |
| The resistance at 60° Fahr.... | ... | ·528$^{\omega}$. | |
| " ... ... | = | 12·898$^{\omega}$ per mile. | |
| " ... ... | = | 1·389$^{\omega}$ per foot grain. | |
| | | Breaking Weight. | Stretch %. |
| The tensile strength, No. 1 Test | ... | 1,189 lbs. | 15·5 |
| " No. 2 " | ... | 1,110 " | 15 |
| Torsion 6 inch length, No. 1 " | ... | 13½ twists. | ... |
| " " No. 2 " | ... | 10½ " | ... |

There is not a trace of galvanization left. The sample was much rusted, so that it was difficult to gauge it accurately, but 175 mils is a fair mean of the diameter, and it shows that the original wire was a full size No. 8—indeed, nearly a No. 7. It shows also that the 36 years' action of the atmosphere has had but a very small destructive effect for the thin coating of oxide upon it is perhaps the only loss it has sustained. It probably retains its original weight. This rust makes it impossible to get at its exact resistance per foot grain, for it adds to the weight without reducing the resistance. The wire that is now supplied on the Post Office specification ranges between 1·13$^{\omega}$ and 1·15$^{\omega}$ per foot grain, while the wire previously supplied, without specifying resistance per mile, used to be 1·51$^{\omega}$ per foot grain. This sample measures 1·389$^{\omega}$, which is probably too high, owing to the oxide formed, but it shows that the wire must originally have been remarkably good, and that its 36 years' continuous service has not affected its electrical qualities.

All the mechanical tests also show that it must have been of excellent quality. The stretch is notable after having been swinging for so many years. The tensile and torsion test-pieces having been

taken, one of each from each end of the spare length, that is from points where the stress being greatest would cause a considerable reduction in the original ductility of the wire. It is still quite equal to the wire we purchase now.

Anyhow, the result of the examination is to show conclusively that 36 years of incessant service in conveying the currents used for ordinary telegraphy have not materially altered the molecular structure of the iron wire. The brittleness arising from the sudden conduction of powerful currents of electricity does not appear to be produced by the long continued application of the minute currents used in telegraphy in iron wire.

What the effect in copper wire may be remains to be found out. Schröder van der Kolk stated that the resistance of copper varies when weak currents have passed through it for some time, but Matthiessen tried to verify this fact without success. His experiment, however, lasted for only six days, and the current was a constant one from two Bunsen cells.* It has often been asserted that the insulated wire of apparatus becomes brittle after long and continued use, but I have not yet succeeded in obtaining any authentic fact to verify this statement.

This is a very useful field for enquiry by the members of the Society. The measurements and tests referred to in this paper were made for me by Mr. Andrew Bell.

Mr. STEARNS: Said he had had very little experience with iron wire, but had been informed when purchasing brass or copper wire, that either kind became brittle after hanging, without currents of electricity ever being brought near it.

Mr. STROH: Agreed with the foregoing, and said that the brittleness produced in brass and copper wire in a short time by simple atmospheric action was almost incredible. Had had no experience with iron wire.

Mr. ADAMS: Asserted that a brass spring in his possession, which had been simply hanging in a dry place for some time, was easily snapped owing to its brittleness.

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* B.A. Report, 1863, p. 56.

Mr. VON TREUENFELD: Asked what was the average duration of iron wire in England.

The PRESIDENT: It depends entirely upon the locality. In smoky places or in the neighbourhood of manufactories where the air is saturated with chemicals (as at Widnes, near Liverpool) galvanised iron wire comes to an untimely end in a very short time after its erection. In clean agricultural districts, such as Hampshire, where the atmosphere is untainted with deleterious gases, iron wire would last, he could not say how long. The coil before them had been up for thirty-six years, and was practically as good as the day it was put up. Locality regulated the durability of uncovered wire.

Mr. VON TREUENFELD: Asked the question because the question of durability seems to him to be a chemical one. He thought it would be very interesting to have an analysis of iron wire that had lasted a length of time, to ascertain in what respects the samples which had stood the best differed from the rest, and then chemically to obtain a quality of iron of uniform durability.

Mr. A. BELL verified the remarks of Mr. Stearns and Mr. Stroh, as he had noticed that a coil of copper wire hanging in a workshop, subject only to the ordinary atmospheric action of such a place, and without being used for the purpose of conveying currents of electricity, became brittle, but from what cause he had not been able to find out. As to the durability of the wire, and in answer to Mr. Treuenfeld, he stated that the better the quality of the iron wire, the better it could be galvanized, and its durability so prolonged. In places where the air was full of sulphurous or other acids the galvanized coating was quickly destroyed. The sample referred to in the note had probably been erected in a place where the air was comparatively pure.

Professor HUGHES explained recent experiments he had made which touched upon this point. Steel wire or rods of about 1-16th diameter were placed for a short time in a solution of sulphuric acid containing 10 per cent. of acid. These became almost as brittle as glass. Other pieces being partially immersed were brittle in that part only that had been in the acid, the other part

remaining perfectly pliable—numerous trials gave invariably the same result. Iron wire of about $\frac{1}{8}$th of an inch thick remaining in the solution for about fifteen minutes was acted upon in the same manner, and to such an extent that it became quite rotten. Wire having been made brittle by such action could be restored to its former ductility by the flame of a spirit lamp. On mentioning this curious fact to Mr. Chandler Roberts, that gentleman supposed that the brittleness was due to an absorption of hydrogen: individually, he was inclined to think that the change was one of molecular structure; future experiments, however, would more clearly give the reasons, but there was no doubt as to the fact itself. To ascertain whether an electric current would have a similar effect upon wires, two steel wires were placed in a glass of water and a current passed through them. Bubbles of hydrogen were given off, but although the current was kept on for some time, no difference whatever appeared in the wires. Thus, acid produced brittleness, which was not the case with a battery current.

The PRESIDENT: The general theory given to account for the rapid decay of wire in the neighbourhood of towns is, that the sulphurous acid in the air combines with the oxide of zinc formed on the wire, producing a sulphate of zinc which, being soluble, is washed away by the first shower of rain, leaving the action to be repeated and continued till decay is complete. With regard to wire used indoors, he might say that he had suffered from the disastrous effects of brass wire used for hanging pictures becoming brittle and prematurely giving way.

A vote of thanks to the President for his note was proposed by Professor HUGHES, who said it was very gratifying to find that iron wire lasted so long. He had been under the impression that it deteriorated more rapidly, and had never heard of such a case of longevity of wire. He had often noticed battery wires breaking, and attributed it to some molecular change taking place in the copper wire. He had not investigated the matter, but it was very satisfactory to know of a case of long life of wire such as brought to notice by Mr. Preece.

Mr. WILLOUGHBY SMITH, in seconding the vote of thanks,

remarked that his experience of iron wire was limited, but that of copper and alloys somewhat extensive. He had reason to believe that wires made from alloys of metals did change with time, and often became brittle. He had a large resistance coil, consisting of fine German silver wire covered with gutta percha, which, after being in use for several years, became defective, and on being examined, the wire was found to be brittle throughout its entire length. Dr. Matthiessen supplied him with a standard resistance, made of wire drawn from an alloy of gold and silver. This soon failed on account of the brittleness of the wire.

It was a well-known fact, that if due care was not taken in the manufacture of wires, even of pure metal, especially in the annealing, they would be brittle; but a wire properly manufactured in the first instance—whether of iron or copper—would not, under ordinary circumstances, become so. If submitted to a high temperature and sulphurous fumes it lost its tenacity, and it was well known that a copper wire covered with india rubber, and subjected to the vulcanizing process, was almost sure to become brittle after a very short time.

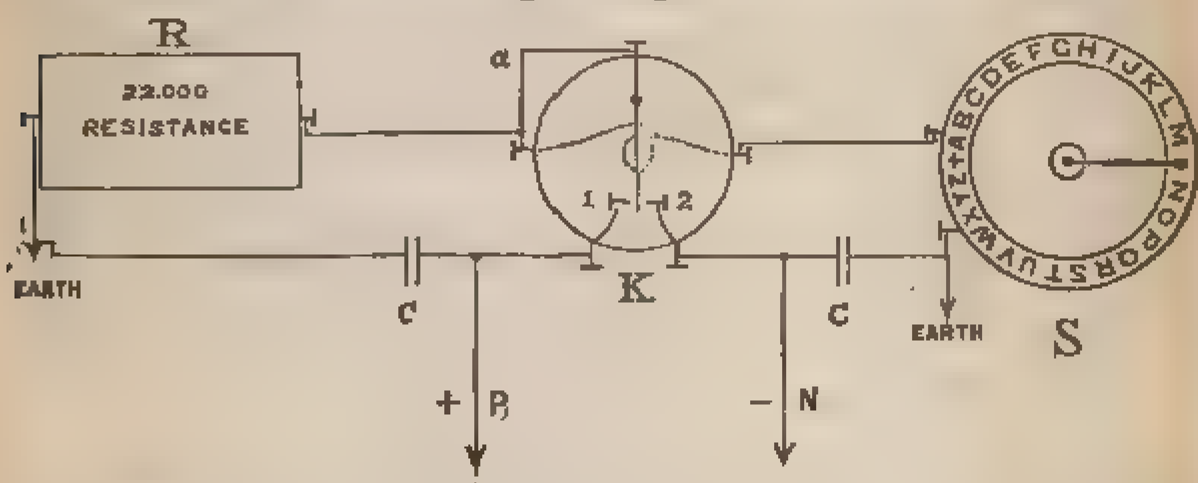
The PRESIDENT: I will just add another fact in confirmation of what I have previously said, and that is, that nearly all the wire erected in the year 1844-5 over the South Western, South Eastern, and Great Eastern Railways is still up; and, indeed, ever since the introduction of telegraphs, the only wire which has required renewal through decay is that erected in the neighbourhood of towns and manufactories where the air is full of acids: in the open country the original wire put up in the early days of telegraphy has attained a good old age, and perhaps much of it will still exist when we do not.

The vote of thanks was carried by acclamation, after which the SECRETARY read the next paper,

# MORSE SIGNALLING BY MAGNETO - ELECTRIC CURRENTS.

By A. EDEN, *Associate.*

About the end of 1878, while experimenting with a different end in view, the idea suggested itself that the magneto-electric transmitting apparatus of an ordinary alphabetical signalling instrument might be made to furnish two distinct and continuous currents of opposite signs, and of sufficient strength to work a Morse circuit either on the single or the double current system. A commutator to reverse the connections at the proper moment would, of course, have effected the separation of the positive and negative currents; but as this, from circumstances, was inadmissible, the following arrangement was made:—




A Siemens' "communicator," S, a resistance, R, of about 22,000 ohms, and a "Stroh" relay, K, were joined up in circuit—earth being used at each end—as shown in the diagram.

The "communicator" contained, as is usual, several permanent magnets and a Siemens' armature rotating between their poles. The rotation produced, of course, alternate positive and negative currents through the circuit, and kept the tongue of the relay in continual vibration between its back and front contact-points, 1 and 2.

By connecting these contact points to separate external circuits, and joining a wire, *a*, from the main circuit to the relay-magnet itself, it is obvious that each time the magnet made contact with either stop it offered an alternative path, or shunt, to the current

then existing, which would be "positive" when touching one of the stops, and "negative" when touching the other.

A very brief interval must elapse between the establishment of (say) a "negative" current and the disconnection of the magnet from the "positive" stop, which, no doubt, allows a minute quantity of the reverse polarity to escape from each "stop," but this amount is so small that (practically) we get a very rapid succession of "positive" currents, or impulses, from one point, and of "negative" currents from the other.

To fill up the minute interval between these currents and to increase the "quantity" if possible, a condenser, *c*, was attached to each of these diverging circuits, the discharge from which occurs while the tongue of the relay is passing to the opposite contact-point.

In effect, then, we have a continuous "positive" current coming from one relay-stop, and a continuous "negative" from the other. These currents can be used for double-current signalling, or independently for single-current transmission on different lines.

The current coming from each relay-stop was found equal to about 100 Daniell cells, with an armature speed of (13 × 110) 1,430 revolutions per minute.

The "positive" and "negative" wires are here quite independent of each other; while using one, the other may be disconnected, or to earth, without affecting the current flowing through the wire in use, the force being thus equal to 200 Daniell cells, if used separately for single-current transmission.

By joining the "positive" and "negative" wires to a post-office "double-current" key, messages were sent from Edinburgh to London (on a wet night) with as much facility as if by galvanic electricity.

On another occasion, in frosty weather, a loop of 308 miles of number eight gauge wire was made up, and, using the "negative" wire alone, signals were transmitted by single current, and good marks were registered (without any local relay) on a "Direct Writer" of the usual pattern.

The current received *from the* "positive" or "negative" stops

was equal to 22 milliwebers (without the condenser only 18·8), and after passing through this loop of 308 miles, 11 milliwebers were received. This does not mean that a loss of 11 milliwebers occurred on the line, but that the resistance of the loop line referred to was nearly equal to the apparent internal resistance of the generating circuit. The added line resistance, and the leakage at the insulators, together reduced the current to 11 milliwebers.

The manual labour involved in the rotation of the armature-coil prevented the trials being extended to automatic transmission at high speeds, and when, as in this case, the driving-wheel rate of revolution frequently varied from 96 to 110 per minute, the results were satisfactory. The rate of rotation and the rapidity of the contacts had a considerable influence on the amount of current produced.

At this stage the attention of your present President was directed to these experiments, and he carefully verified these observations, using steam-power, similar instruments and connections.

Mr. Kempe, who conducted the experiments, states that "with a speed given to the armature of about 500 revolutions per minute, two circuits of 3,000 ohms resistance each could be worked perfectly. The current obtained from the machine, on short circuit, when running at the rate of 400 revolutions per minute, was 18 milliwebers. At 200 to 300 revolutions this was increased to 18·5, but with a higher velocity, viz., 500 revolutions, the current given was 18 milliwebers, showing that at a very high velocity the current tends to diminish, probably from the action of the relay not being so prompt as at a lower speed."

This system being thus shown to be practical, the question is simply whether magneto-electric currents are less expensive than galvanic currents while equally effective. It appears to the writer that, by introducing a magneto-electric machine worked by steam-power, with a separate Siemens' armature for each circuit (or for each set of local circuits), a great saving would be effected. The armatures could be made of different resistances and gauges of wire, to suit the resistance of the circuit to be worked.

As the wires from each armature could be led separately to a

"battery board" as at present, power could be increased, or a faulty armature removed, with as much ease as if batteries were employed, and, if found desirable, condensers could be attached as in these experiments.

One horse power would probably be enough to replace 2,000 cells.

Deschanel says that 3 horse power is sufficient for the very powerful lighthouse machines supplied by the French "Compagnie l'Alliance," while a speed of armature rotation of 1,500 to 2,000 per minute is necessary in Wilde's machine (Siemens' and Gramme's machines we work at much less speed), so that neither the required engine power, nor the speed of revolution would overstep the limits already reached by existing machines.

It would certainly seem more advantageous in every way that the machine should consist of many small and independent armatures, so as to obviate the necessity for dividing the current from one large generator.

The usual spring-contacts on the armatures, so as to produce continuous currents, would doubtless be preferable to the relay arrangement, although the "positive" and "negative" wires would not then be independent of each other.

It may be said that the telegraphic superiority of zinc as compared with coal has been decided long ago, but the separate armature idea here advocated, the improved insulation of our lines at the present day, and the great advance in magneto-electric machines, make it desirable that we should look anew into this question.

When the cost of sulphate of copper or other salt, of transit of stores and renewal of plates, is considered, there seems no reason to doubt the superiority of coal.

Before closing this paper, it may be observed that the resistance offered by moving contacts is worthy of investigation. The current obtained from the rapid contacts of the arrangement now reported on was observed by Mr. Kempe to attain a maximum at a medium rate of revolution. This might at first sight appear to have some connection with the magnet's natural rate of oscillation depending on its mass, &c., and the question is complicated by the fact that the electromotive force produced would vary with the rate of revolution,

but even in automatic transmitters driven by a falling weight, there is a point at which increase of speed increases the resistance, and lessens the current in circuit. At lower rates the current is not sensibly increased until motion ceases, when the current is at a maximum.

A perfectly clean double-current key in motion offers a resistance of about ten ohms, while a Wheatstone transmitter has a minimum of about fifty. Increase of electro-motive force lessens the resistance, while even a small amount of oxidation largely increases it, and renders it very variable.

In submitting this paper to the Society I am perfectly aware that magneto-electric currents have been used for working Morse circuits by Siemens, Wheatstone, Wilde, and others, but I venture to hope that there is sufficient novelty in the way in which this question has been proposed and tried to justify its being brought before the Society.

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The President: In proposing the vote of thanks to Mr. Eden for his paper, I would remark that the employment of magneto currents for battery currents is no new thing in itself. In the year 1865, Mr. Varley tried a Wilde's machine at the Electric Telegraph Company's Central Station, in Telegraph Street, and worked for some time several circuits from it. Again, in 1872, Mr. Culley gave a very exhaustive trial of a Gramme machine at the Post Office Central Telegraph Station. Failure resulted in both cases from the difficulty of maintaining the resistance of the points of contact at the commutator constant. Great variations in currents were observable, and some trouble caused in consequence. Owing to the experience gained in these trials, and experiments made in Manchester, on the Lancashire and Yorkshire Railway particularly, Mr. Wilde, in 1878, took out a fresh patent for working circuits by means of magneto-electric currents, his plan being to have excited by the same primary current a series of secondary coils, which varied in resistance according to the circuit they had to work. Mr. Wilde not having had an opportunity of trying this plan on a large scale at present, it has never been practically carried out. Mr. Schwendler, too, in the December,

1879, number of the *Philosophical Magazine*, has contributed a very interesting paper, showing how two circuits (from Calcutta, I think) were worked by means of surplus currents (they may perhaps be called) from a machine used for an electric light. The current used for lighting purposes is something like 30 webers, the current used for working a Siemens relay is about 3 milliwebers, so that the mere abstraction of the ten-thousandth part of a current cannot possibly influence its working, nor can it be felt. Such an experiment was no doubt very interesting. The use of these currents has been a very favourite idea with our transatlantic friends. When I was in America nearly three years ago the subject was discussed. In New York, the battery room of the Western Union Company is at the top of a very high, handsome, though not a very well built building, and contains nearly 15,000 cells, which of course represent a very heavy weight. It became a matter of importance to remove this weight, and the expedient of employing magneto currents was called to mind. Mr. Stephen Field, of San Francisco, experimented with it with a great deal of success; so much so that the Western Union Company determined to replace all their 19,000 cells—5,000 being in another building—by 15 Siemens' machines of the small type. They are arranged in a series of four (one set being spare),

| | | | |
|---|---|---|---|
| The first giving an electro-motive force of | | | 50 volts. |
| The second | ,, | ,, | 50 ,, |
| The third | ,, | ,, | 50 ,, |
| The fourth | ,, | ,, | 100 ,, |

The fifth being used as a feeder to the others.

So that a variable electro-motive force is available of 50, 100, 150, or 250 volts, and by arranging these machines in series they are able to work the whole of their circuits from the Central Station. The first object they had in view, of course, was to remove the dangerous weight of the batteries—over 60 tons. The second was to economise space. I have said more than once in this room that in America they are more extravagant with their battery system than we are. In their Central Station, with about half the number of circuits, they have as many cells as we have; their cells cost them nearly ten times as much per annum to maintain, and the amount of *material wasted in their* pet form of battery, the gravity,

is excessive. Of course the greater length of their circuits is one reason for this, and another their closed circuit system of working. In determining the question of introducing magneto machines, it is simply a matter of pounds, shillings and pence; of coal *versus* zinc; and it is quite possible, with carelessness and waste, to consume so much zinc that the use of coal would become economical. But it should be remembered that in a battery the whole of the energy of the zinc is usefully consumed; in an engine of the very best type it is only possible to use about ten per cent. of the energy of the coal. Batteries such as we use in England require little attention; with engines there is a necessity for constant supervision. A battery is silent and motionless, while an engine is noisy and full of friction. A battery eats only when it is hungry: an engine is always hungry and is always eating. A battery is constant and regular, while an engine is variable and uncertain. The result is that where batteries are economical in their consumption and properly worked, then it is a very easy thing to show that it is far more economical and efficient to work a station by means of batteries than by means of engines. It is a subject which received very careful attention from the Post Office some seven or eight years ago, but which, owing to the recent prominence given to it by the Western Union Company, has led some people to imagine that again our Yankee friends are ahead of us, and that in using magneto-electric currents they have made a fresh start and shown us a new path; but I can assure the Society that if it had been found eight years ago that there was any economy or advantage in using the Gramme or Siemens' machine at the Central Station the system would certainly have been adopted. Before sitting down, I propose a vote of thanks to Mr Eden for the paper which he has kindly furnished us.

Seconded by Mr. Willoughby Smith, and carried.

The SECRETARY then read the following paper:—

## COMPENSATING INDUCTION IN PARALLEL WIRES.

By CHARLES H. WILSON, Chicago.

I HAVE read with much interest the researches of Professor Hughes, *elaborated in his paper* on "Induction," which has been presented

and discussed before the Society of Telegraph Engineers, and I have had an earnest desire to acknowledge to him the kindly manner in which he afterwards noticed my efforts, and credited to me what I have accomplished.

I am to-day in receipt of the latest number of that Society's Journal, and notice in its records a detailed account of the discussion.

Professor Hughes in referring to my system for compensating induction, says—"Mr. Chas. H. Wilson's very beautiful and superior arrangement will perfectly compensate for two lines. The arrangement I propose will compensate for all lines, each adjacent wire being perfectly free from induction."

In the respect of my plan being applicable to two lines only, I fear the Professor is labouring under a misapprehension. In the brief article which I prepared for the American Electrical Society, and from which all of the printed accounts of my invention were taken, I did not think it necessary to describe the arrangement any further in detail, but now I see that it would have been more favourably considered if I had done so.

There being no difference in the general plan whatever, when more than two lines are compensated for, any description of this nature was thought superfluous.

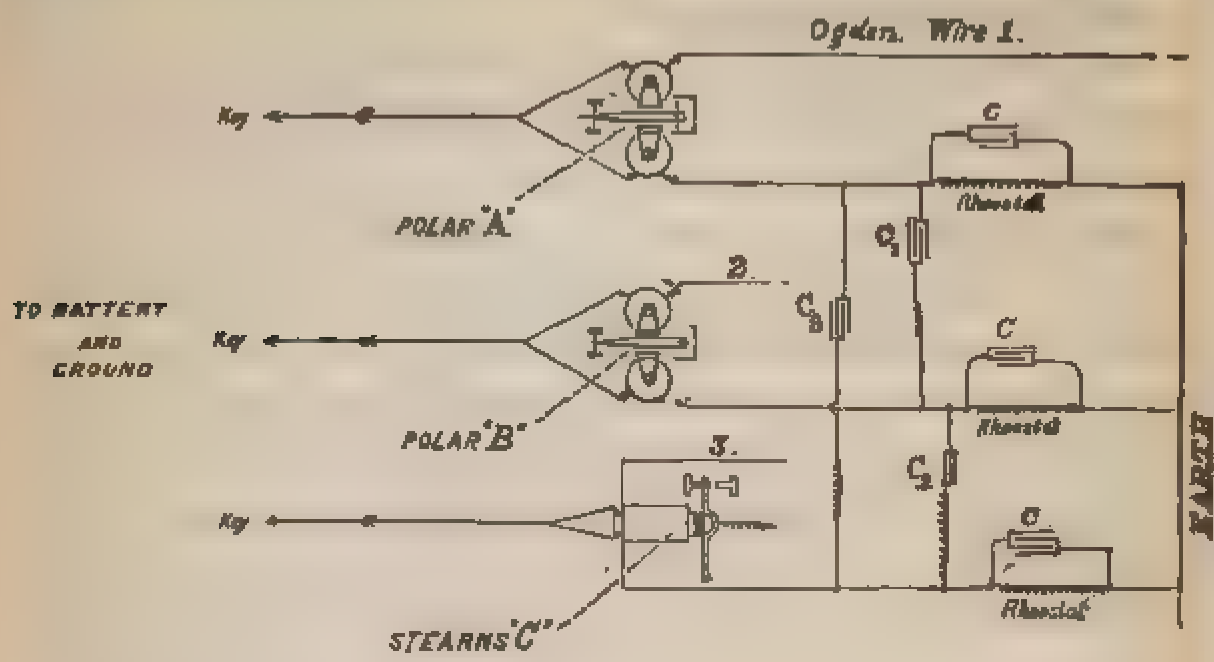
When several different lines are to be compensated, in practice we balance, or equate, two of them without regard to the others; after this has been done one of these and a third are adjusted, and so on until all the wires are similarly treated.

My arrangements differ slightly according to circumstances, being adapted to the precise conditions of the lines from which the interference arises. Between Ogden (Utah) and Cheyenne (Wyoming), a distance of over five hundred (500) miles, we work three wires duplexed, two of them on the polarized relay or double current plan, and the third in the ordinary manner, with a neutral relay.

The "differential" duplex is used on the lines of the Western Union Co. exclusively.

In consequence of the wires all being of the same gauge, and in other respects very similar to each other, a very simple arrange-

ment is employed, which is shewn in the following symbolical diagram, by which it will be seen how closely the inductive action of the three lines is imitated with reference to the return current.




The three condensers C, C, and C, are connected around the main rheostats of the respective sets of instruments, and with the rheostats form their "artificial lines."

Condensers $C_1$, $C_2$, and $C_3$, in combination with resistance coils of 10,000 ohms, are used to balance the return induced current.

You will notice that each artificial line has two *active* condensers; the third always remaining neutral when the first and second charge and discharge. The two active condensers of each set of duplex, discharge respectively, into the "artificial lines" of the other two sets, while the inactive condenser, owing to the potential of its two sides being equal, remain silent.

Suppose, for illustration, a current is sent out (or the battery already on the line, reversed), upon the wire which in the diagram I have termed "Polar B," then, that part of the current from the battery which passes through the "artificial line" of this set will charge the plates of condensers $C_1$ and $C_2$ to a certain potential depending upon the capacity of the condensers, the electromotive force of the current flowing, *and the resistance of the artificial line*, and, as a consequence, momentary currents will issue from the other plates of the condensers, and a portion of these currents will flow through the second coil of the differentially wound relays.

It is obvious that when that portion of the condenser discharge flowing through any relay is made equal to the return induced current coming back simultaneously through the other coil of the same relay, the resultant action upon the magnet core will be *nil*, and practically neither the induced currents from the line, nor that from the condensers, will be perceptible. When the above action takes place and condensers $C_1$ and $C_2$ charge, it might properly be said that $C_3$ would be in the "bridge," and in consequence of the "four sides" having the requisite values to satisfy Wheatstone's equation, in a neutral position regarding the currents under consideration.

In reference to the conditions involved in the charging of the condensers, I have underlined a portion of the sentence, "and the resistance of the artificial line."

I wish to call particular attention to this factor in the essential adjustment of the apparatus.

It plays a part in the practical working of my apparatus which at first thought may appear but a trifling matter, but after a moment's reflection upon the everyday working of wires, with continually varying atmospheres of escape, I doubt not that a point which has been of so much importance to us will be appreciated.

Duplexes are necessarily *re-balanced* with every atmospheric change that materially alters the *apparent* conductivity resistance of the wires, in order that the conditions of the artificial line will be made to conform closely with those of the actual line.

Now, if it were found necessary to increase the delay already occasioned in balancing by further necessities of the same nature arising in the induction apparatus, the time lost would be more than trebled and this would be greatly to the detriment of business.

In all of the induction apparatus we have in use the adjustment required is only occasional; the ordinary changes in the "artificial line" circuit, necessarily made from time to time, also suffices for the induction condensers. When, it matters not from what cause, a line is more heavily laden with escape, and as a consequence the return currents diminish in magnitude, to avoid interruption from the passage of the outgoing currents the resistance of the rheostats forming a part of the "artificial lines" is diminished.

The effect of the diminution of this resistance lessens the inductive action of the condensers in two ways; first, by lowering the potential of the "artificial line" at the point of contact where the condensers receive their charge; secondly, by increasing the conductivity of the derived circuits which act as shunts to the helices of the relays, with reference to the currents coming into the artificial lines from the condensers when the latter are acted upon.

This feature, I think, does not exist in any other form of induction compensating apparatus that has come under my observation.

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Professor HUGHES: The description of Mr. Wilson's arrangement, as given in Mr. Prescott's book, describes only its application to two lines, and I thought very highly of the arrangement, notwithstanding the fact that if used for several lines, such as ten, it would become too complicated to be of real service. (Professor Hughes here explained by a diagram on black board the difficulties arising when attempting to compensate for induction on several wires.) Mr. Wilson has a great many condensers in his three wire arrangement, but, as I have just explained, for a line of ten wires something like forty-five compensating condensers would be required. It was this multiplication of condensers that compelled me to be cautious in saying that "with two lines it would be a beautiful and perfect arrangement," but with any number of wires greater than two the matter becomes very complicated.

Professor AYRTON: It occurs to me that Mr. Wilson intends to refer to the compensation of electro-static induction, whereas Professor Hughes' remarks refer to electro-magnetic induction. With either kind of induction the effect would be the same at the sending station. A positive current passing along a line connected at the other end to earth would charge that wire positively, and consequently there would, due to electro-static induction, be a rush of negative electricity into any neighbouring wire from both ends. The same effect, as far as the sending end is concerned, is produced by electro-magnetic induction. If a positive current is sent along a wire an opposite current is set up in the neighbouring wire, that is, negative electricity rushes into the neighbouring wire at the

sending end. It may, consequently, be possible to arrange a system, either on Professor Hughes' or Mr. Wilson's plan, to compensate for both kinds of induction at the sending station, but neither would compensate for electro-static induction on a neighbouring wire at the receiving end. On the other hand, Professor Hughes' has this advantage, that it will compensate for electro-magnetic induction all along the line, whereas Mr. Wilson's can only produce a compensation at the sending end.

I do not propose to enter further into this question this evening, but I hope those who are making daily experiments on the telegraph lines will tell us whether their experience shows that it is electro-static or electro-magnetic induction on neighbouring wires that is the most to be avoided.

Mr. STEARNS asked for information as to the variation of induction caused by atmospheric variation, particularly in reference to compensating condensers. Everyone knew that in duplex working the condenser had to be decreased or increased when the weather was wet or dry, but perhaps it was not so well known that sometimes the capacity had actually to be *increased* in wet weather.

In reply to a remark made by Professor Perry (but which was subsequently withdrawn) to the effect that theory would tend to show that the electro-static induction from line to line was small compared with the electro-magnetic.

The PRESIDENT said: Theory and practice sometimes differ. Practice brings into play a great many conditions that theory cannot always account for. In working parallel wires both kinds of induction are present. A current passing along a wire charges its neighbour up to a certain potential, which continues as long as the current flows, but on the cessation of the latter its inductive power ceases, and the charge raised along the whole of the neighbouring wire proceeds to earth—two-thirds, or nearly so, going in one direction, the remainder in the other direction; and it is that portion falling back, as it were, in the same direction as the working current that causes retardation in working. Such effect is so strong in places (as between Teheran and Kurrachee) that it is always possible to read on the induced wire that which is being sent on the proper one. The same effect

is experienced between New York and Chicago, where two wires are parallel for about 800 miles. This shows that the effect of electro-static induction in neighbouring wires is not so insignificant as may be imagined, and that the direction of the current due to it, as pointed out by Professor Ayrton, is, at the receiving end of the line, contrary to that due to electro-magnetic induction, and similar to the course of the working current. Mr. Wilson has not attempted to compensate for this, on account of the extreme difficulty of doing so, as pointed out by Professor Hughes. What he has attempted to compensate is the return current, as he calls it, or the discharge due to electro-static induction at the sending end, and he has done so to a practical extent. Professor Hughes and Mr. Wilson have arrived at very near the same results at the sending end, but have been combating different foes—Professor Hughes has attacked electro-magnetic, and Mr. Wilson electro-static, induction, which he has no doubt perfectly cured for two or three lines, as regards the effects of the peculiar conditions met with at Chicago. Nobody has yet attempted to compensate for static induction at the receiving end, and I am afraid that the problem is so difficult as to make success doubtful, and even then the game may not be worth the candle.

We are always glad to get papers such as Mr. Wilson's, which invite discussion, and which always do good in bringing out weak points and clearing away wrong impressions.

A hearty vote of thanks was then passed to Mr. Wilson for his paper.

A ballot for new members took place, at which the following were elected :—

*Foreign Member :*

Frederic A. Gower.

*Members :*

| | |
|---|---|
| John B. Saunders. | Adolphe Pierre Vassard. |

*Associates :*

| | |
|---|---|
| Robert Bayly. | Clement Auguste Lepelley. |
| William Gilhespy. | Charles Porter. |
| Edouard Louis Lalonde. | John Smith. |

*After which the Meeting* adjourned until February 25th.

# ABSTRACTS.

## OLIVER HEAVISIDE—ON THE EFFECT OF FAULTS ON THE SPEED OF WORKING OF CABLES.

(*"Philosophical Magazine," Vol. 8, No. 46, pp. 60–74, and No. 47, pp.* 163–177, 1879.)

A leakage fault in a cable, besides weakening the received currents, has another effect, viz., to accelerate the rapidity of increase and decrease of the currents, or more generally the rapidity of change from one state to another, thus enabling the cable to be worked at a higher speed. The charge produced by a continued E.M F at one end is more quickly established with the aid of a leak; for the initial charge, which is nothing, may be produced by the superimposition of two charges of opposite sign, each numerically equal to the final charge produced by the E.M.F. Let one of these discharge itself, then the cable will become charged with the other; and since the fault, by decreasing the resistance to discharge, accelerates the dissipation of the one charge, it must equally accelerate the establishment of the other. An artificial conductive connection of constant resistance between the conductor and sheathing of a cable, preferably at its centre, would greatly increase the working speed—however objectionable a natural leak may be.

The author investigates mathematically the effect of a fault on the size of reversals, with numerical illustrations, when the ends of the line are to earth, and when they are insulated. The strength of received reversals is not so much lessened by a leak as a permanent current. The general theory of any initial charge with the usual terminal connections and a number of leaks is also investigated. Particular cases are worked out numerically, and diagrams of the arrival curves of the current at one end due to a constant E M.F. at the other, when a leak is situated at the centre of the line, are given; (1) when both ends of the line are to earth, and (2) when they are insulated (the latter corresponding to condenser working); also (3), the arrival curves of the potential in the latter case. In each instance, three values of the resistance of the leak are taken, zero and infinity, and one-fourth of the cable's resistance for the intermediate value.

Other problems in connection with a cable are also considered, and the solutions in the case of arbitrary terminal conditions, for which the original may be consulted.

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## ALUMINIUM FOR TELEGRAPH LINES.

(*"Les Mondes," No. 6, February 5th,* 1880, *p.* 251)

Experiments have recently been made by the German Telegraphic Administration regarding the *employment of* aluminium in place of iron wires, in

consequence of aluminium having twice the conducting power of iron. Of course the high price of the former material would prevent the line wires being made entirely of that substance, but experiment has shown the possibility of making an alloy of iron and aluminium, which, although containing but a very small percentage of the latter substance, has a high conductivity, and so the line wires may be made of smaller diameter than before. The alloy is also tough, and better able to resist the destructive action of the atmosphere than iron.

## KÜLP—EXPERIMENTAL ENQUIRY INTO MAGNETIC COERCITIVE FORCE.

(*"Carl Repertorium für Experimental-Physik,"* B. xvi. *H.* i., *p.* 45.)

### First Paper. On the Remanent Magnetism of Soft Iron, and the Influence of Vibrations on it.

This is a continuation of Dr. Külp's researches published in *Poggendorff's Annalen*, from 1868–1875. He has undertaken a series of researches on coercitive force in hard and soft iron as well as in hard and soft steel, and in this first paper he begins with the consideration of cylindrical bars of soft iron, especially with reference to the effect of vibration upon remanent magnetism. The diameters of the iron and steel rods which he used varied from 1 to 4 millimetres, and their lengths from 50 to 160 mm. To magnetize them he could use one or other of two coils of different lengths and internal diameters, and his battery power was from one to four Bunsen's elements. Coil No. 1 had only one series of windings of 200 turns; No 2 had eight layers of 129 turns each. It is evident that the magnetization of the rods, in the centres of these coils, was produced in uniform magnetic fields.

The current being made, the induced magnetic moment is measured with a Wiedemann's magnetometer and afterwards, the current being broken, the remanent magnetic moment. The rod is now taken out of the coil, and either, as we shall find in further papers put for a time in a quiet place in an east and west position, or else subjected to vibration, and then again the remanent magnetic moment is decided. The vibration of the rod was caused by dropping it six times from a height of 1·5 metres upon an earthen floor. He used the following rods: E and F were of commercial iron wire. The four first were also of commercial iron wire, but they had been surrounded with clay and heated for several hours within a coal fire, and let lie to cool for half a day.

| | Length. | Diameter. |
|---|---|---|
| A | 160 | 1·0 |
| B | 100 | 1·0 |
| C | 50 | 1·5 |
| D | 50 | 4·0 |
| E | 160 | 2·5 |
| F | 160 | 2·5 |

The following table of results for bar A will show the curious difficulties of the investigation.

| Magnetizing Power. | First Time of Vibration. Induced Magnetism. | First Time of Vibration. Remanent Magnetism before Vibration. | First Time of Vibration. Remanent Magnetism after Vibration. | Repetition of Experiment. Induced Magnetism. | Repetition of Experiment. Remanent Magnetism before Vibration. | Repetition of Experiment. Remanent Magnetism after Vibration. |
|---|---|---|---|---|---|---|
| 17 | 16 | 5 | 3 | 14 | 6 | 3 |
| 35 | 56 | 18 | 2 | 55 | 23 | 2 |
| 54 | 88 | 33 | -5 | 77 | 28 | 2 |
| 73 | 108 | 23 | 3 | 108 | 29 | 7 |
| 93 | 138 | 37 | 6 | 138 | 36 | 7 |
| 113 | 159 | 24 | 7 | 155 | 32 | 6 |

From the want of proportionality between induced and remanent magnetism in this and many other cases, the author comes to the conclusion that in remanent magnetism certain intrinsic forces receive significance which in induced magnetization either disappear or exert another influence. The one instance of negative magnetism observable in the table is often seen in other rods, and shows itself in different experiments with the same rod, without there seeming to be any decided law for it. Ordinary remanent magnetism does not seem to follow any clear law, unless that, as is already known, it becomes less in shorter rods, whereas the values of it after vibration are of particular significance. Thus, considering the percentage of loss of remanent magnetism due to vibration—

Rod A showed with coil No. 1, after three times repeated magnetization (with magnetizing power 17) and vibration, the losses 37, 44, and 60, when the rod was very distant from its point of saturation. With magnetizing power 93 the losses after three repetitions were 83, 79, and 91.

Rod E with coil No. 2 with small magnetizing power, in six repetitions showed losses 57, 64, 64, 67, 64, 60; with medium magnetizing power 76, 71, 69, 75, 79, 73; and with strong magnetizing power 83, 84, 77, 70, 76, 75.

Rod F showed under the same circumstances as the last just given for Rod E, 93, 90, 88, 86, 86, 84.

It appears, then, that with small magnetizing power, repeated magnetization and vibration cause an increase in the loss due to vibration. This agrees with the magnetic molecular revolution theory, for the molecules cannot wholly return to their original positions on account of internal forces, such as friction. Vibration brings them nearer to their equilibrium positions, and therefore decreases remanent magnetism. Repeated magnetization and

vibration cause the molecules to become more mobile and the vibration losses must become greater. It appears, however, that this is not the case with greater magnetizing powers, and the author concludes that friction and other internal forces then annul one another or act in a different sense. The vibration loss is certainly greater with greater magnetizing power. Again, it is seen that the shape of a rod plays an important part in the phenomena. Thus the vibration loss is greater in short rods than in long ones.

## NIAUDET—CELL WITH BLEACHING POWDER.

("*Journal de Physique*," *No.* 97, *January*, 1880, *p.* 18.)

M. Niaudet has devised a new cell with zinc and carbon plates. The zinc plate is immersed in a solution of sea salt, containing 24 per cent. of the salt, which proportion is found to impart maximum conductivity to the solution, and the carbon plate is put in a porous cell with chloride of lime. The only products arising from the action of this cell are water and chloride of lime; this salt, as is well known, being one of the most soluble bodies. No insoluble salts are produced by the secondary action of the cell.

At the commencement, the electromotive force is 1·6 volts. The cell is not completely depolarised by the chloride of lime, for it polarises if used to send a current through a small resistance; which effect, however, to a great extent, passes off if the cell is left for some time inactive, but it never regains its first electromotive force.

The chief merit of M. Niaudet's cell is that there is absolutely no action in the zinc when the cell is not sending a current. To prevent the unpleasant smell arising from the chloride of lime, the cell is closed with a waxed cork with a small hole for pouring in the water to start the action when the cell is put up.

## E. H. HALL—ON A NEW ACTION OF THE MAGNETS ON ELECTRIC CURRENTS.

("*American Journal of Mathematics*," *Vol. II. p.* 287.)

The following experiment had apparently been tried by Prof. Rowland, but without success :—

"A disc or strip of metal forming part of an electric current was placed between the poles of an electro-magnet, the disc cutting across the lines of force. The two poles of a sensitive galvanometer were placed in connection with different parts of the disc, through which an electric current was passing until two nearly equipotential points were found, the magnet current was then turned on and the galvanometer was then observed, in order to detect any indication of a change in the relative potential of the two poles."

No such change could be observed, and Mr. Hall now repeated the same experiment substituting a piece of gold leaf mounted on glass for the metal strip. Experimenting as above on October 28th, he obtained a decided deflection of the galvanometer needle.

"This deflection was much too large to be attributed to the direct action of the magnet on the galvanometer needle, or to any similar cause. It was, moreover, a permanent deflection and therefore not to be accounted for by induction."

Some rough quantitative experiments were tried with the result "that with a given form and arrangement of apparatus the action on the Thomson galvanometer is proportional to the product of the magnetic force by the current through the gold leaf. This is not the same as saying that the effect on the Thomson galvanometer is under all circumstances proportional to the current which is passing between the poles of the magnet. If a strip of copper of the same length and breadth as the gold leaf, but ½ mm. in thickness is substituted for the latter, the galvanometer fails to detect any current arising from the action of the magnet, except an induction current at the moment of making or breaking the magnetic circuit."

# JOURNAL

OF THE

# SOCIETY OF TELEGRAPH ENGINEERS.

VOL. IX. 1880. No. 31.


The Eighty-fourth Ordinary General Meeting of the Society was held on Wednesday evening, February 25th, 1880, at the Institution of Civil Engineers, 25, Great George Street, Westminster—Mr. W. H. PREECE, President, in the chair.

The minutes of the previous meeting having been read, and the names of new candidates announced—

The PRESIDENT said: Before commencing the items set down for this evening I have one remark to make, which is perhaps rather of a personal nature.

In the address that I had the honour of reading before you, on the 28th January, I said that "the telephone—the great excitement of the previous session—has made but little progress during the past year, though there is no doubt that through the discovery of the principle of the microphone by Professor Hughes the practical application of that wonderful instrument had been much improved. In fact, Professor Graham Bell, Mr. Edison, Mr. Elisha Gray, and all those who have been working in this field have laid aside their form of transmitter, and have adopted one that is a mere form of microphone."

I find, on inquiry, that the statement I made that Mr. Edison has abandoned his own particular form of transmitter and used a form of microphone is not strictly correct. The form of transmitter now used by Mr. Edison differs but very little from that which he used before the introduction of the microphone, and as so many

controversies are arising on technical points connected with telephones, I think it most essential that I should not err on any point of fact, whatever opinions I may hold. Such opinions are my own, and I am quite free to express them, but facts are public property. I have, and have expressed, strong opinions in the matter of telephones and microphones, but I readily confess that in my address I made a mistake on this particular point, and, therefore, I propose to alter the sentence that it may read thus—"In point of fact, Professor Graham Bell, Mr. Elisha Gray, and most of those who have been working in this field have laid aside their own particular form of transmitter, and have adopted one that is a mere form of microphone."

To proceed with the business of the evening, I will thank any gentleman for his observations on Mr. Eden's paper on "Morse Signalling by Magneto-Electric Currents."

No remarks, however, in addition to those made on the previous occasion being offered, the meeting proceeded to the continuation of the discussion on Mr. Wilson's paper—"Compensating Induction in Parallel Circuits."

Mr. J. T. Hill said that in reply to a question from Mr. Stearns at the last meeting, if any variation had been noticed in this country in the capacity of the condenser required in different states of the weather, he would remark that there was a noticeable difference. It did not appear that the variation was uniform or followed any particular rule, but the general tendency was that less condenser was required in wet than in fine weather. This would be seen in the following observations taken :—

| | | Feb. 12, 1880 | Feb. 14. | Feb. 18. | Feb. 19. | Feb. 21. | Feb. 23. | Remarks. |
|---|---|---|---|---|---|---|---|---|
| | | Weather fine after rain. | Weather wet. | Weather fine. | Weather Showery. | Weather dull, threatening rain. | Weather wet. | |
| Duplex. | LIVERPOOL, *via* road ... | 3·75 | 2 75 | 3·25<br>4360 | 3·25<br>3200 | 3·75<br>5040 | 3.<br>3120 | |
| Duplex. | LIVERPOOL, *via* G. W. Railway. | 4 75 | 3·75 | 4·25<br>4400 | 4·25<br>3600 | 4·75<br>4000 | 5·75<br>4320 | |
| Duplex. | NEWCASTLE ... ... .. | 4·25 | 3· | 3 75<br>5200 | *3·<br>4400 | 4 25<br>5040 | 3·<br>3760 | * Started at 3·75 and R=5040. Afterwards wet, and reduced to 3· and R=4400. |
| Duplex. | EDINBURGH ... ... .. | 3·5 N T | 2·1 | 3.<br>4600 | 3.<br>2880 | 3.<br>4520 | Not working. | Working through a relay at Newcastle. |
| Quadruplex. | MANCHESTER ... ... ... | 2·75 | 2·88 | 2 43<br>3880 | 2·75<br>3120 | 2·88<br>3920 | 2·13<br>2760 | |
| Quadruplex. | LIVERPOOL, *via* N. W. Railway. | 3·5 | 3· | 3·27<br>4560 | 3·25<br>3320 | 3·75<br>4640 | 3·02<br>2960 | |
| Quadruplex. | LEEDS ... ... ... .. | 3·5 | 3·3 | 3·81<br>3520 | 3·61<br>3000 | 3·68<br>3880 | 3·05<br>3520 | |
| Quadruplex. | BRISTOL ... ... ... ... | 3 | 3·07 | 3 11<br>3180 | 3·<br>2840 | 3·07<br>3440 | 3·07<br>2720 | |
| Quadruplex. | BRISTOL ... ... ... ... | 2·5 | 2·18 | 3·25<br>2560 | 3·<br>2200 | 2·9<br>2600 | 3·25<br>2520 | |
| Quadruplex. | SOUTHAMPTON . ... ... | ·75 | ·5 | ·75<br>1920 | ·75<br>1480 | ·75<br>1840 | ·75<br>1600 | |

The first line of figures shows the capacity of the condenser in microfarads.
The second line the resistance (where noted) of the line in ohms.

In the later observations the resistance of the line was also noted and recorded; but beyond the fact that there was a general tendency in the same direction, in both the resistance of the line and the capacity of the condenser used, there did not appear to be any uniform rule governing the two together, the resistance being sometimes comparatively high when the condenser was low, and *vice versa*; but still the tendency was the same in both—to rise in fine, and to fall in bad weather.

Mr. WILLOUGHBY SMITH asked if the absolute resistance of the line could be given, as well as the capacity of the condensers used on the days named, as he believed that the variable resistance of the line would account for the variation required in the capacity of the condenser for duplexing in wet weather, as mentioned by Mr. Stearn.

As regards Mr. Wilson's paper he (Mr. Willoughby Smith) was sorry he had to leave the last Meeting while so important a subject was being discussed by three such eminent men as Professor Hughes, Professor Ayrton, and Professor Perry, but from what he had heard of the discussion there appeared to be a confusion of terms which prevented him from always grasping the meaning of the speaker. He would illustrate on the black-board the several forms of induction as he understood them, and, if he was in error, perhaps those more conversant with the subject would kindly put him right. He then showed how the induction of a coil of insulated copper wire immersed in water in an insulated tank could be accurately measured in six different ways. First, by connecting one pole of the battery to a copper plate immersed in the water in the insulated tank, the other pole of the battery being attached to one terminal of the galvanometer, then, when the other terminal of the galvanometer was brought into contact with one end of the coil, the other end of the coil being free, a deflection would be observed on the galvanometer. This he understood as charge. Secondly, if one terminal of the galvanometer was connected to earth, and the other terminal brought into contact with the end of the coil immediately after the coil had been charged, the needle of the galvanometer would be deflected to the same degree as in the first experiment, but in the opposite direction. This he understood

as discharge. Thirdly, the galvanometer could be connected in the circuit of the copper plate in the water, and, then, on the coil being charged the needle of the galvanometer would be deflected as before. But it was needless to proceed further, as it would be readily seen the various ways there were to obtain the same result, and whichever way the result was obtained he understood the effect to be electrostatic induction, and the sole cause of retardation in the working of submarine cables. The President in that able address which he had so recently read, had said that Faraday's "Experimental Researches in Electricity" ought to be every electrician's bible. Mr. Smith agreed that it was no doubt "the book" in which to obtain a comprehensive knowledge of induction. If two coils of insulated wire or two parallel wires were brought into close proximity to each other, and a current sent through one, an instrument in circuit in the other would be affected on either making or breaking the battery circuit. This he understood as an induced current. The effect of such a current was manifested while laying the cable between Brest and St. Pierre in 1869. The 2753 knots of cable were coiled in one continuous length into three tanks on board the "Great Eastern," the first end was taken into the test room on board, and 900 knots coiled in the main tank, then 921 into the after tank, and 719 into the fore tank, and then the 213 remaining knots were coiled on the 900 knots in the main tank, the end being taken to the shore station. When the cable was charged, or its electrical condition in any way changed, the two coils in the main tank acted inductively on each other, the consequence being that when shore gave the continuity signal a deflection was immediately received on the ship's galvanometer, due to the induced current, followed after an appreciable time by the true current. The effect of the first current was christened "The Ghost," and it continued until the 213 knots were laid, when it entirely disappeared. If a length of wire forming the circuit of a galvanometer be passed quickly through the magnetic field a deflection on the galvanometer would be observed. This phenomenon being more marked in a coiled cable on shipboard, especially when the ship rolled freely. In fact, by careful observations during the laying of a cable the electrician at the shore

station could estimate the extent of the roll. This he understood to be true magnetic induction. Aerial wires would be influenced more or less by static induction, by induced currents, and by true magnetic induction, and knowing the variable character of each, he could readily understand the difficulties attending any system of compensation. Mr. Smith then described a recent experiment he had made which illustrated the electrical and other properties of gutta percha. A bladder of that material 203·6 square inch area, and ·00065 of an inch in thickness, containing 12 lbs. 14 oz. of water, was suspended in an inverted glass shade containing water; a copper wire, in contact with the water in the bladder, and passing through the neck of the same, was connected to one pole of a battery of 100 cells, the other pole being connected to one terminal of an astatic mirror-reflecting galvanometer, the other terminal of the galvanometer being connected to a copper plate immersed in the water in the glass shade. The absolute resistance of the gutta percha after one minute's electrification was 1487·6 megohms, and after ten minutes 4103·8 megohms. The polarization or electrification was regular and uniform with both currents, in fact, its behaviour was the same as that of cores of submarine cables. The absolute inductive capacity was ·2266 microfarads, equal to about 1600 yards of core containing 107 lbs. of copper and 140 lbs. of gutta percha per nautical mile.

Professor AYRTON: Looking at the very interesting table exhibited by Mr. Hill, of the various resistances and capacities necessary to give to the artificial lines in duplex working, it will be seen that on the whole small capacity accompanies low resistance, and that both are found to exist together on damp days. But I do not think from this we can conclude whether or not the absolute capacity of a perfectly insulated land line would increase or decrease in wet weather. In duplex working, the capacity of a line means the rush-out of electricity when a battery is disconnected from a line, the other end of which is to earth through the bridge or other duplex signalling arrangement; and since, from defective insulation, the line becomes practically shorter in wet weather than in dry, we might expect the rush-out to be less in the former case than in the latter.

But although from the adjustments used in duplex working it is very difficult to say whether or not the specific inductive capacity of air alters with the weather, there ought to be no difficulty in deciding this question experimentally; for the combination of the three tests on any wire, the conduction test, the insulation test, and the discharge test with the other end of the line insulated, furnish materials for calculating approximately the actual capacity of the line at any particular time, and a comparison of these capacities on different occasions would decide the matter.

As regards the vexed question, the relative importance of electro-static and electro-magnetic induction in interfering with the working of telegraph lines, I think there can be little doubt that these *two* kinds of induction do really exist from wire to wire. When a current is sent into a telegraph line, say, a positive current, then all parts of that wire are charged to a positive potential, which is less and less as we approach the other end of the line. All portions of any adjacent wire which has one or both of its ends to earth will consequently be charged negatively by *static* induction. On discharging the first wire, a rush of negative electricity will take place from both ends of the second wire, if both ends of this wire be to earth. On the other hand, when the current is stopped in the first wire, then due to *electro-magnetic induction*, there will be a momentary positive current set up in the second wire, that is, a rush of positive *into* the secondary wire at the sending end, and a rush of positive electricity *out* of the secondary wire at the receiving end. At the sending end, then, the effects of static and of electro-magnetic induction of one wire on an adjacent one are combined, while at the receiving end they are opposite in character.

Since the last meeting, I have made a short calculation* as to the relative amounts of discharge at the receiving end produced in a wire by electro-static and electro-magnetic induction from a neighbouring one through which a battery current is sent and stopped, and I find, if $E$ be the electro-motive force of the battery, $l$ the length and $r$ the radius of either wire, $d$ the distance between the secondary and the primary, $L$ the resistance of the secondary wire,

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* Given at the conclusion of the discussion, page 81.

and $R$ the resistance of the signalling arrangement at either end of the latter, that the quantity of electricity discharged at the receiving end of the secondary wire, by the cessation of *electro-static* induction when the current in the primary is stopped and the primary therefore discharged, is roughly

$$\frac{E\,r}{8\,d\,v^2}\,l\,\frac{\frac{L^2}{3}+L\,k+R^2}{(L+2\,R)^2}$$

in electro-magnetic units, $v$ being the ratio of the electro-magnetic unit to the electro-static unit of electric quantity. On the other hand the discharge from the sending end of the secondary, due to electro-magnetic induction when the current in the primary is stopped, is

$$\frac{2\,l\,log\,\frac{h}{d}\,E}{(L+2\,R)^2}$$

$h$ being the height of the primary wire from the ground, the parallel secondary being at a distance $d$ vertically underneath it.

The ratio of these two quantities is, therefore,

$$\frac{r\left(\frac{L^2}{3}+Lk+R^2\right)}{16\,d\,v^2\,log\,\frac{h}{d}}$$

From this we see that, if the radius of the wires be small or if the lines be short, the discharge from a line due to the electrostatic induction of a neighbouring wire will be much less than that arising from the electro-magnetic induction, but if the wires be thick and long, then electro-static induction from wire to wire will be far more important than the electro-magnetic. And I find that the former becomes roughly six times the latter when the lines are 500 miles long, composed of No. 8 iron wire, and when a signalling arrangement having a resistance of 1,000 ohms is employed at each end of the secondary.

Mr. ADAMS thought that, whereas electro-static induction came into play during and after the passage of a current, electromagnetic induction took effect during the precise interval between different electrical conditions,—in fact, at the moment of *make* or *break* of a circuit,—and that, practically, the electro-magnetic

effect was of so short duration, as compared with the static, that disturbances due to the former were far less perceptible.

As regards the effect of weather upon the static condition of wires, there really existed no criteria. It sometimes occurred that damp weather, whilst reducing the insulation of a wire, appeared to increase its static capacity, although a continuance of rain would again reduce it—an anomaly unaccountable to him, unless it might be assumed that a larger electrical superficies was temporarily given to the wire by the formation of a coating of damp, greasy, dust.

Professor AYRTON remarked that it was exceedingly probable that the electro-static capacity of a telegraph line increased in wet weather; for suppose that, from floods, the level of a river rose until it nearly was as high as that of a telegraph line, then there could be no doubt that the capacity of the portion of the line over the river would be immensely increased. Now, although perhaps merely damp air might not have a very different specific inductive capacity from dry, analogy would lead us to conclude that air actually containing globules of water must necessarily have a much higher specific inductive capacity, and that, therefore, the true capacity of an aerial line during rain would be much higher than in dry weather.

Mr. C. A. MORGAN gave an instance touching upon the point in question. One morning recently, about ten o'clock, while balancing a London-Liverpool wire of No. 8 gauge, the weather being fine, the resistance of the circuit was 4,400 ohms, and electro-static capacity 3·5 microfarads. A little later in the day, about 12 noon, rainy weather prevailing reduced the resistance to 2,000 ohms, but no alteration in the condenser was necessary, as, the electro-static capacity of the line appeared to be the same, as having a differential galvanometer in the circuit for balancing purposes, any discharge from the line would soon have shown itself. A similar instance occurred when the above wire was crossed with a No. 4 gauge wire yesterday: the rest of the circuit was reduced, but the electro-static capacity remained the same, viz., 3·5 microfarads.

In regard to the effects of electro-static and electro-dynamic

induction, it was worthy of remark, that in the form of transmitter originally used on the first introduction of Wheatstone's automatic apparatus, certain effects were noticed which prevented a constant high rate of speed being attained. Intermittent currents were at that time employed. Mr. Culley's compensating arrangement was introduced, and the ill effects disappeared. Subsequent improvements in the receiving apparatus have enabled working with permanent currents through long distances at higher speeds than were previously thought attainable. The cause of this improvement appeared to be due to the construction of the electro-magnet in the present form of receiver, and that, whereas in the old form of receiver the armature was made by means of two small permanent magnets with their reverse ends adjacent, in the form now in use the electro-magnet and armature were shortened and induced by an adjacent permanent magnet. (A detailed explanation was given of the magnetic action in both forms of receiver.) The question was whether, owing to the fact that the cores and armature were polarised by a more powerful magnet, the electro-magnet induced current, supplemented by the magneto-electric induced current, were of such strength and direction as to oppose, and consequently neutralise the effect of the retardation current following the arrival of the actual signal current.

Professor HUGHES: The discussion has deviated from the original subject of Mr. Wilson's paper, which treats of the induction felt upon the second wire, and not that on the transmitting wire itself. It is not static induction, which is well known, but the electro-magnetic induction which is felt on wires near the transmitting wire in countries possessing dry atmosphere, such as the United States and France. Faraday studied both kinds of induction, and made experiments in each direction. In countries possessing dry atmosphere the magnetic field surrounding a wire along which a current is passing makes itself inconveniently felt on its neighbouring wires; and Mr. Wilson deserves great credit for having brought before us his solution of a great and increasing practical difficulty experienced upon long aerial lines in dry climates. It would have given me more pleasure if our discussion on this subject could have brought out some still more practical

remedies, but the discussion has wandered off into theoretical views, which, however beautiful in themselves, does not bring, nor even suggests, a remedy for lateral induction.

The question is a very difficult one, and a Commission has been sitting in France since 1868 attempting to thoroughly solve it, but up to the present time no solution has been arrived at. For whilst it is comparatively easy to neutralize the induction by condensers and similar remedies, they introduce at the same time an extra cause of difficulty in working, which, in most cases, makes the remedy worse than the disease.

I do not despair, however, of seeing some day a perfect resolution of this problem, and our best thanks are due to Mr. Wilson for having not only attempted a solution, but, as regards two lines alone, practically succeeded.

Mr. SPAGNOLETTI explained difficulties he had experienced in 1870 on long wires, owing to this induction. Complaints of contact reached him. Thorough search with an astatic galvanometer for fault was made, nothing was found. Bad earth wires were suggested as the cause. All earth wires were renewed, but still the effect continued: signals by automatic instruments escaped from their proper wires on to their neighbours and caused confusion. The Post Office authorities then caused experiments to be made, and their officer, Mr. Marson, reported induction as the cause. The effect was ultimately got over by changing the positions of the wires on the poles.

Professor AYRTON disagreed with Prof. Hughes's view that the action of one telegraph wire on an adjacent one was only electro-magnetic; for he held that, when a current was started or stopped in a wire, the electro-magnetic field was undoubtedly changed in the neighbourhood; but while the current was flowing there was an electro-static field, and the effects of the two fields on a neighbouring line must be taken into account in any calculation of inductive disturbance from wire to wire. It also appeared to him, before discussing the different forms of remedies, to make quite sure what exactly was the nature of the evil that had to be cured.

The PRESIDENT felt convinced that the electro-static capacity of the air did vary between extreme weathers. This circumstance

was easily accounted for in theory, but became most difficult to prove in practice. He had tried almost every conceivable experiment in the matter. Condensers had been placed in aqueous vapours of various density, but no reliable result as to the extent of variation had yet been arrived at. The results obtained from duplex working were not convincing; but one phenomenon was invariably present during wet weather which prompted the supposition that signals were more retarded in consequence. The tailing-off of signals and diminished rate of working of fast speed apparatus might be due to other causes than variation of capacity, but there was every reason to believe that such effects were due either to polarisation on the insulators or to the variation of the electro-static capacity of the air. Such a question might be solved by members of the society vigilantly noting all variations of capacity and tracing their cause.

The subject of electro-static and electro-magnetic induction was brought before the Society in 1872, and the experiments referred to by Mr. Spagnoletti were then related. At that time Mr. Culley and himself investigated the two phenomena, and the result arrived at was that in open wires in England the effects of electro-magnetic induction was practically nothing as regards working, while electro-static induction very detrimentally manifested itself.

The nature of either kind of induction was quite different from the other. Its direction at the further end differed in each case. If two wires running side by side be taken, and a current sent along one, it would at once be seen, on the second wire, whether the effect be electro-static or electro-magnetic; and such effect agreed with Professor Hughes' proportion of one-tenth. Professor Ayrton had shown that the two kinds of induction are always present, and must perforce be present; but there was this difference between the two, that while in the case of electro-magnetic induction the effect diminished with the square of the distance between the two wires, in the case of electro-static induction it diminished only directly with the distance, and where such disproportion existed it was clear that one power must soon overcome the other.

Mr. Morgan had thrown out a very interesting suggestion as to

whether currents of induction set up by apparatus itself might not, to a certain extent, counteract the induced current. That point was worth examining, and one that no one was in a better position to prosecute than Mr. Morgan himself.

Mr. Willoughby Smith had referred to a third species of induction—the true magnetic induction. This was very apparent on board ship, and also when testing a cable from shore to ship. But it was also felt at times in open land wires when they were swinging through the lines of the earth's magnetism. But such effects were only felt on the most sensitive of instruments. Experiments performed by himself with a telephone attached to an insulated wire, totally free from electrical disturbances, had shown currents to exist that were inexplicable, except on the assumption that they were produced by the vibrations of the wire cutting the lines of the earth's magnetism.

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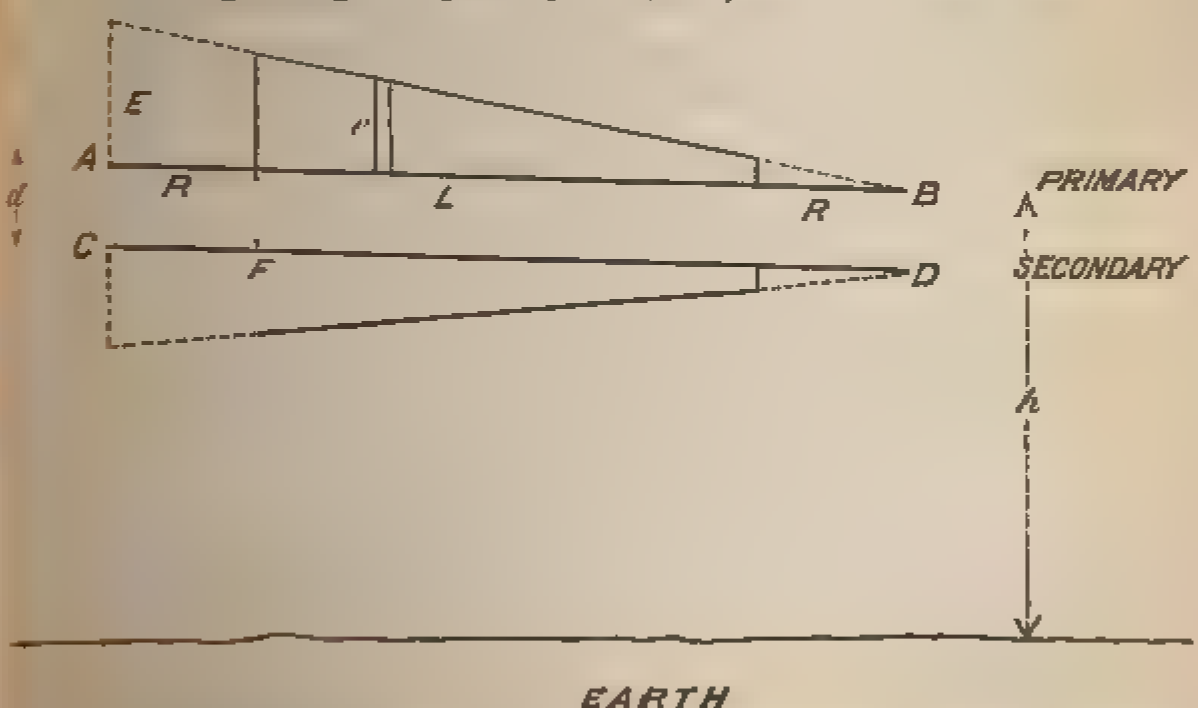
## NOTE ON THE ELECTRO-MAGNETIC AND THE ELECTRO-STATIC INDUCTION FROM WIRE TO WIRE IN TELEGRAPH LINES.

By W. E. Ayrton and John Perry.

(*See page 75 of the Discussion on Mr. Wilson's Paper.*)

Let $c$ be the current in electro-magnetic units (centimetre, gramme, second), flowing through the primary wire, A B;




Let $l$ be the length of either the primary or secondary telegraph wire, in centimetres;

Let $d$ be the distance between the two wires, in centimetres;
Let $h$ be the height of the upper from the ground, in centimetres;
and for simplicity of calculation, let the two wires be in the same vertical plane, then the number of lines of force due to the upper circuit which cut through the lower is

$$2\, c\, l \log \frac{h}{d}$$

If then L be the resistance of the secondary line C D, and R the resistance of the instrumental arrangement at each end of the line, the quantity of electricity, in electro-magnetic units (gramme, centimetre), that will flow through the secondary telegraph wire, due to the starting or stopping of the current $c$ in the primary, is

$$\frac{2\, c\, l \log \frac{h}{d}}{L + 2\, R}$$

if the instrumental arrangements at neither end of the lines have any inductive effect on one another.

The coefficient of electro-static induction per centimetre length of one wire on the other will, if the distance from either to the earth be great, compared with their distance asunder, be about

$$\frac{r}{8\, d}$$

in centimetre, second electro-static units, where $r$ is the radius of the wire in centimetres. Hence the static quantity of electricity, in electro-magnetic units, induced in any length $dx$ of the secondary wire at a distance $x$ from F is

$$e \frac{r}{8\, d\, v^2} dx$$

where $v$ is the ratio of the electro-magnetic to the electro-static unit of electric quantity, or equal to $298 \times 10^8$, and $e$ is the difference of potentials between the earth and the point in the primary wire opposite that in the secondary wire of which we are speaking.

If $\rho$ is the resistance per unit of length of the line wire,

$$e = E \frac{(l - x)\, \rho + R}{L + 2\, R}$$

where, as before, $l$ is the length of the line, therefore the static charge in any element $dx$ at a point distant $x$ from F is

$$E \frac{(l - x)\, \rho + R}{L + 2\, R} \times \frac{r}{8\, d\, v^2} dx$$

When the current in the primary wire is interrupted, and the primary wire therefore discharged, this quantity of electricity will flow out partly at one end and partly at the other of the secondary. The fraction which discharges itself at the sending end will be

$$\frac{(l - x)\, \rho + R}{L + 2\, R} \text{ of the preceding,}$$

ence the total discharge from the secondary wire, flowing out at the sending nd, due to electro-static induction from the primary, is

$$\frac{E\,r}{8\,d\,v^2}\int_0^l \left\{\frac{(l-x)\,p+R}{L+2\,R}\right\}^2 dx$$

n the assumption that there is no electro-static induction from the instrumental rrangement on one line to that on the other at either end of the lines.

And remembering that $l\,p$ equals L, we see that the above becomes

$$\frac{E\,r}{8\,d\,v^2}\,l\,\frac{\frac{L^2}{3}+L\,R+R^2}{(L+2\,R)^2}$$

Hence the ratio of the discharge arising from electro-static induction to that produced by electro magnetic induction is

$$\frac{r\left(\frac{L^2}{3}+L\,R+R^2\right)}{16\,d\,v^2\log\frac{h}{d}}.$$

As an example, let each line be 500 miles long, and composed of No. 8 iron wire, having a resistance of 14 ohms per mile, and a radius of 0·085 inches or 0·215 centimetres. Let the vertical distance between the two wires be one foot, or, say, 30·5 centimetres, and the upper wire 12 feet, or 366 centimetres, from the ground; and let R, or the resistance at each end of the line, be 1,000 ohms. Then L is equal to 500 × 14 × $10^9$ absolute electro-magnetic units of resistance, and R to 1,000 × $10^9$, so that the preceding fraction becomes equal to about six.

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The Secretary then read the following paper:—

## ON A FAULT IN THE CONSTRUCTION OF DIFFERENTIAL INSTRUMENTS.

By J. B. Stearns.

Probably every one who has made much use of differential measuring instruments, and especially of Thomson's Differential Mirror Galvanometer, has observed that a practically perfect *balance*, or equality of action between the two wires, is not difficult to obtain in the first instance, but that this balance is not constant, the action of one wire or the other almost always predominating.

There are, I believe, more causes than one for this loss of balance; for example, if, in measuring resistances with the differential galvanometer, we carelessly allow the current to flow

for a moment through one wire only, or if, as is almost inevitable, the current in one wire is for a time and until the rheostat can be adjusted, stronger than that in the other, the temperature, and consequently the resistance of the wire carrying the stronger current is increased, the balance of the instrument itself is disturbed, and the result of the measurement is not trustworthy. This, however, is not the case when the instrument is first joined in parallel circuit, without external resistance, and the current simultaneously passed through the two wires in opposite directions. In this latter case the currents in the two wires commence to flow at the same instant; the resistance of the wires and the strength of the current in each wire should be, and I believe are, exactly the same, and still we often find that the balance is not perfect. In practice I have always found it necessary to add external resistance on the side of stronger action. But if, as I am convinced, this defect in the instrument is not due to any change in the relative resistances of the wires, this adding of external resistance in order to restore the balance is at best a vicious makeshift, and the results obtained by the use of an instrument thus balanced are not reliable. It is scarcely supposable that when the instrument has not been used for some days the temperature of one wire can be higher than that of the other, or that a change of temperature, however great, should affect one wire more than the other, unless, indeed, the quality of the copper of which the two wires are composed is different.

About a year ago I thought that I had found one cause for this variation in the differential galvanometer, and I communicated my observations, and the inferences I drew from them, in a letter to our President, who has been so kind as to ask me to prepare a short paper on the subject to be read here to-night. Upon consideration I have thought it better to read the letter itself without additions or alterations.

"Some weeks ago I had a mirror galvanometer wound differentially and very carefully adjusted, first by sending a current of 20 U. K. cells through one wire in one direction, and back through the other wire in the contrary direction, as in Fig. 1, and adding turns of

wire on the weaker side until the effect was *nil*. It was next joined up in parallel circuit, as shown in Fig. 2, and external resistance

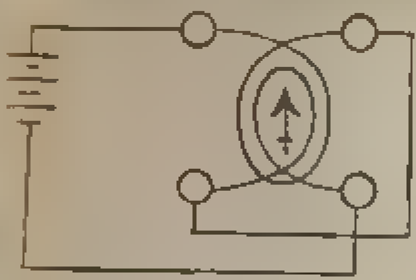

FIG. 1.

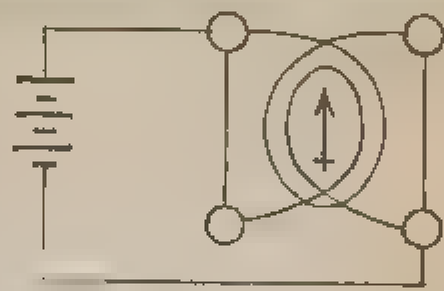

FIG. 2.

added to one of the wires until again the effect was *nil*. This galvanometer gave satisfactory results, until, on February 9th, it was found that when it was tested, as in Figs. 1 or 2, it was unbalanced, and the spot of light moved. Several turns had to be taken off one of the wires to again make it balance accurately. This time, in adjusting the balance, another tube, containing a mirror much more sensitively suspended, was employed. When it had been perfectly adjusted again, I removed the more sensitive mirror, and put its own mirror in its place, when it was found to be much out of balance. After looking in all other directions for the cause of this peculiar action, it was discovered that if the tube carrying the mirror was pushed in or drawn out a trifle it changed the balance—*i e.*, if it was pushed in the thirtieth or fortieth of an inch, the spot would move, say, to the right, and if it was drawn out a like distance, the spot would move to the left! This action is somewhat puzzling. If we suppose that in the differential galvanometer there are two magnetic fields of exactly the same extent and intensity but of opposite polarities, then all parts of the magnetic field are neutral, or, strictly speaking, there is no magnetic field at all. Then why this action on changing the position of the magnet and mirror?

"As an explanation I offer the following:—In winding any differential coil it is the usual practice to wind the two wires from separate bobbins, holding the two wires firmly between the thumb and finger, and allowing the wires to slowly traverse the coil from right to left, and back from left to right again, and so on until the coil is finished. Now, although the wires become crossed occasion-

ally, still on the whole the wire from the right hand bobbin will be to the right on the coil, and nearer the right hand end of the coil than the other wire by at least the diameter of the wire; but as it constantly happens in winding that the two wires become separated to the extent of a tenth of an inch or more, this separation of the two wires, the right hand wire to the right, and the left hand wire to the left, becomes still more decided. To make my meaning plainer, suppose the two wires to be of exactly the same size, but the right hand one to be platinum and the left hand one of aluminium. Now, when the coil has been evenly wound, if it could be accurately cut in halves through the centre transversely, the right hand half would be the heavier.

"Now, if we make a differential galvanometer of two separate *coils,* as shown in Fig 3, and put a current through one coil in one

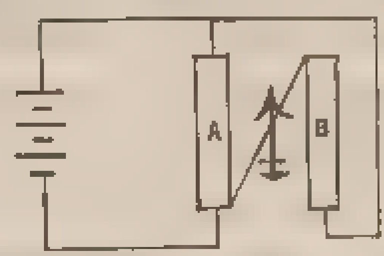


Fig. 3.

direction, and through the other coil in the other direction, it is obviously of importance that the magnet be in the centre between the two coils, for if the magnet be moved towards coil A the influence of that coil will predominate over that of B. Now does not an ordinary *two wire* differential coil to some extent resemble the *two coil* differential in the distribution of its two wires, when constructed as above described?

"If this view is the correct one, this fault in differential instruments may be obviated by changing the relative position of the two wires at every turning, *i.e.*, the wire that was to the right while winding the first layer should be to the left in winding back on the second layer, and so on. Perhaps the same correction could be secured by twisting the two wires together before or as they were wound, but this would be objectionable on several accounts.

"J. B. STEARNS.

"Valentia, February 12, 1879."

I will only add to the letter the further observation, that in order to produce this want of balance in the differential mirror galvanometer, it is not necessary to change the position of the tube within the coil, but that a few turns of one of the levelling screws in order to bring the "spot" to the required height on the scale, will produce the same result, especially when the mirror (and magnet) are suspended at the top only. This renders it the more important that this defect in the construction of differential instruments should in some way be obviated. I will also observe that I have, since the date of the above letter, taken advantage of this very defect to obtain a perfect balance in an instrument otherwise only approximately adjusted; for example, when the instrument is not properly balanced with the mirror in the centre of the coil, a perfect balance may be obtained by withdrawing the tube or pushing it in a little; but I believe a balance thus obtained is a fictitious balance, and not to be relied upon for accurate measurements.

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Mr. KAISER agreed with Mr. Stearns, and believed that the method proposed in his paper would produce as true a differential as possible.

Professor AYRTON: Have you abandoned the plan of spinning the silk simultaneously on two copper wires side by side, so as to make a kind of double wire for differential winding?

Mr. KAISER: Almost totally. Such wire is only used for galvanometers of low resistance.

Mr. A. LE NEVE FOSTER also agreed with Mr. Stearns. The difficulty in differential winding was the impossibility of getting two wires in the same position: the nearer that could be done the more perfect the differential. As to the double wire mentioned by Professor Ayrton, a great objection to its use was the difficulty of repairing either wire in case of breakage. Uneven stretching of either of such wires would lead to error in differential instruments.

Mr. STROH related his experience in the winding of electro-magnets, and found it the best plan to measure off lengths of wire required for electro-magnets, and, finding two of equal resistance,

use them for a magnet; but even then it was sometimes difficult to obtain the same number of turns for each coil.

A vote of thanks was accorded Mr. Stearns for his paper.

The PRESIDENT announced that at the next meeting the subject of the Electric Light would be brought forward, and members having lamps or other articles connected with illumination by electricity were invited to send them for exhibition, as all requisite power for a proper display would be at the disposal of the meeting.

The Meeting was then adjourned until Wednesday, March 10th.

The Eighty-fifth Ordinary General Meeting of the Society was held on Wednesday Evening, March 10th, 1880, at the Institution of Civil Engineers, 25, Great George Street, Westminster—Mr. W. H. PREECE, President, in the Chair.

After the usual formal business had been gone through, the following paper was read:—

## ON SOME RECENT IMPROVEMENTS IN ELECTRIC LIGHT APPARATUS.

By ALEXANDER SIEMENS, (*Member.*)

About two years ago a paper was read before the Institution of Civil Engineers by Messrs. Brittle and Higgs, on "Recent Improvements in Dynamo-Electric Apparatus," treating chiefly of the application of dynamo machines for producing light, and many interesting facts were brought forward, not only in the paper itself, but in the discussion following it, to prove that electricity is, as an illuminating agent, far superior to gas in a great number of applications; but it was quite evident, that no extensive use was made of the electric light for practical purposes at that time, as only one or two statements were made as to the actual cost and the results obtained in working the light regularly, the discussion turning chiefly on *theoretical considerations.*

It is gratifying to all those, who have endeavoured since that time to make a general introduction of the light possible by improving and simplifying the necessary apparatus, that it is at present conceded on all sides, that for all purposes, requiring light centres of great power, the electric light stands without a rival as regards cheapness and efficiency.

The attention of the general public, which was directed to the subject of "Electric Light *v.* Gas Light," by the Jablochkoff lights being used in the Avenue de l'Opera in Paris and by the the scare produced by Mr. Edison's telegrams in October, 1878, had at first the effect of inducing speculators in many parts of the country to display the light on various occasions. In this way the light was brought to the notice of many people, with whom only

seeing is believing, and these temporary displays opened the road to a great many permanent applications.

As is the case upon the introduction of any new invention, there were failures and successes recorded and discussed, and the supporters of the electric light had often a hard time with their critics, as the failures occurred mostly on occasions when the light ought really to have shown to its greatest advantage.

During the lecture of our President at the opening of the Electric Light Exhibition at the Albert Hall, the lights of nearly all the systems shewn, behaved in a most unruly fashion, and all holders of gas shares pointed to the fluctuating Wilde lights, to the Jablochkoff candles, which would not light, and to the spluttering Siemens lights, rejoicing that their gas-light was not like one of these. Upon that occasion, as at most of these exhibitions, the installation of the lights was of a very temporary character and did not permit of a direct comparison with the gas-light. In places, however, where proper arrangements were made for the electric light, few failures occurred, and each successful application has brought in its wake a number of others, which could be undertaken with greater confidence, as the practical experience of electric light engineers increased.

The lighting of streets was seriously contemplated and on several important towns applying to Parliament for powers to introduce the electric light on a large scale, a Committee was appointed, which investigated the subject thoroughly with the help of all the prominent scientific and practical men, who had studied the question. The final Report of this Committee recognises the value of the electric light for all purposes, where a *concentrated powerful light* is required, and expresses the opinion, that its future development should be encouraged.

It was a natural consequence of the continually increasing demand for electric light, that the instruments existing at the time should be subjected to severe tests, and their faults made the most of by its adversaries. On the other hand a great many persons found it worth their while to devote special attention to the subject, and they have certainly succeeded in making considerable advances towards rendering the light as manageable and as reliable as gas.

To do full justice to *all* that has been done by way of improvement in the various systems of electric lighting, is far beyond the power of an individual, and it must be left to the members taking part in the discussion, to render an account of the systems with which the author is not as intimately acquainted as they are.

In the paper mentioned above, and on various other occasions, notably in Dr. Hopkinson's Paper read before the Institution of Mechanical Engineers, it has been demonstrated that a dynamo-machine will develop in the form of a current of electricity up to 90 per cent. of the energy expended on it. This result is so excellent, that a further improvement in this respect is hardly to be expected. But in the ordinary dynamo-machines, the intensity of the magnetic field, in which the armature revolves, varies very much, being greatest when the external resistance is smallest and *vice versa*. If, therefore, the lamps producing the light are not working very regularly, their action re-acts continually on the machine in the most unfavourable way by weakening the magnetic-field when the resistance is *greatest* and the current *most* wanted, and by inducing the most *powerful* currents when the *least* resistance is to be surmounted. This defect in the arrangement of dynamo-machines often resulted, during practical work, in the insulation of the wire on the armatures being destroyed by excessive currents, if the action of the machine and of the lamp were not closely watched, and impeded very much the introduction of the machines into general use, as specially instructed workmen were not always easily obtainable.

The ingenuity of inventors has been at work almost from the first day of the existence of dynamo-machines to overcome this difficulty, and, among others, the following plans have been suggested :—

In machines where a number of bobbins revolve between the fixed electro-magnets, the bobbins have been divided into two groups, connected in such a way that the current produced in *one* group serves only to excite the field-magnets, and the current emanating from the *other* group, is utilised in the *external circuit.* This plan was proposed by Mr. Wilde early in 1867, and about the same time Mr. Ladd showed his dynamo-machine at the Paris

Exhibition; this machine was constructed with two revolving armatures, one of which produces the current for creating and maintaining the magnetic-field, and the *other* the working current.

Since that time, a great many variations of these two plans have been patented; but they all labour under the double disadvantage of making the construction of the machines complicated, and of reducing their efficiency.

In his first communication to the Royal Society in February, 1867, Prof. Wheatstone suggested that the entire current should not be allowed to pass round the electro-magnets, but that a *part* of it only should be diverted for producing the magnetic-field For this purpose, comparatively thin wire, having a large resistance, is wound on the electro-magnets, and the connections are so arranged that the current produced in the revolving armature divides in two parts—the *one* traversing the electro-magnets only and returning direct to the helix, and the *other* producing the desired effect *outside* the machine.

At first sight this plan would seem to answer all requirements, for when the resistance of the light circuit increases, the magnets will receive an increased amount of current, the intensity of the magnetic-field becomes greater, and a stronger current is produced; on the other hand, if the carbons touch, or the resistance of the light circuit is diminished by any other cause, the electro-magnets obtain a much smaller share of the current, and the destruction of the insulation by short circuiting is made impossible if the resistance of the electro-magnets is large enough. A further important advantage is that the magnetic-field becomes permanent, which is of great value for a number of applications.

As a further improvement, it has been suggested by Mr. Brush to wind both the main circuit and the high resistance *shunt* circuit *on* the electro-magnets, in order to get the full effect of the current for exciting the magnets; but by so doing, he incurs the danger of burning the insulation of his machine when it is short-circuited.

A few days ago Dr. Siemens communicated to the Royal Society a paper containing some very interesting results obtained by comparing the currents produced by an ordinary dynamo-

machine, and by a machine in which the coils on the field-magnets form a shunt to the main circuit, With his kind permission two diagrams are placed before the Meeting, showing the E.M.F., the strength of the current, the H.P. *absorbed*, and the H.P. *utilized*, when different resistances were placed in the outer circuit. With an increased resistance the E.M.F. of an ordinary machine rapidly diminishes, and its efficiency becomes less, but in the other machine the E.M.F. rises rapidly under the same circumstances, and its efficiency is not so much affected.

Notwithstanding all these advantages this plan presents the grave defect that a disturbance in the resistance of the outer circuit *re*-acts on the intensity of the magnetic-field, and creates a farther fluctuation in the current passing through the whole system. Moreover, a variation in the strength of the field magnets causes a corresponding variation in the power absorbed, and thus renders the task of the governors, regulating the speed of the engines, more difficult ; it also displaces the most favourable point of contact for the brushes, and thus gives rise to their rapid destruction by sparking.

A constant and permanent magnetic-field is, therefore, of paramount importance, and it can be produced in the way proposed by Mr. Wilde in 1863 for magneto-electric machines by employing a separate machine for exciting the field-magnets of one or more similar machines. The exciting machine has, in this case, an invariable resistance in circuit, and will give off a steady current, maintaining a constant magnetic field in the light-producing machines.

It is obvious that the first cost of the plant is augmented when a separate exciter is used, and for applications, where only one or two lights are required, this increased outlay cannot often be recommended. In such cases the machines can be guarded against being burnt by either adopting Wheatstone's shunt-method or by increasing the resistance of the leads to the lamp. There is no doubt that by the latter plan the current will be diminished, and less light be obtained for a H.P., but the fluctuation of the current will be far less violent, than in an ordinary dynamo-machine connected with its lamp through a small resistance. A few figures

will show the difference: the resistance of the arc is about 1 ohm, and usually ·1 of an ohm is the resistance allowed in the leading wires; the external resistance of the machine varies, therefore, from 1·1 ohm, when the light is burning steadily, to ·1 ohm, when the carbons touch; that is to say, the variation is about 90 per cent. of the normal resistance. When, however, leading wires, having the resistance of 1 ohm, are inserted, the resistances, under similar conditions, vary between 2 ohms and 1 ohm, the fluctuation is only 50°/$_{\circ}$ of the normal resistance, and the minimum resistance is still large enough to prevent an immediate destruction of the insulation.

The objection, therefore, that dynamo-machines are very delicate to handle and require skilled attendance, can be met by one or the other of the described methods, and the machines can now-a-days be trusted to anybody who is capable of oiling their bearings.

The *alternate* current machines have stood the practical test even better than the dynamo-machines, which may be gathered from the fact that the arrangement of their parts has hardly been changed since they were first constructed.

An important improvement has, however been introduced by Messrs. Siemens Brothers, who omit in their machines the iron cores of the revolving coils. The heating effect of the cores caused by the incessant reversing of their polarity, is thereby avoided, and the intensity of the magnetic-field scarcely affected.

From all the above remarks it may be gathered, that the machines for producing the electric light, whether dynamo-electric or alternate current, are certainly so far perfected that nothing stands in the way of their being generally employed.

It would be well for the electric light, if a similar satisfaction could be expressed as regards the lamps, from which the light emanates. The very fact that innumerable inventions are continually made in this field, shows, that there is still great room for improvement, and that here we encounter the main obstacle to the introduction of the light.

On looking through the patents recently taken out for such purpose, it is apparent that sufficient advantage has not been taken of the valuable information contained in Colonel Bolton's paper

entitled, "Some Historical Notes on the Electric Light," published in Part 27 of the Society's Journal, by a perusal of which many inventors might have saved themselves considerable trouble and outlay.

The requirements of a lamp, fit for general use, may be pointed out in a few words. Its construction should be simple, so as to make it cheap, and easily kept in order. It should remain alight, without attention, for any length of time, burning either alone or with others in the same circuit. In the latter case the extinguishing of one light should not affect the others, and while being steady and silent, its light should be adjustable

Although these conditions seem hard to fulfil, several proposals have been made, which come very near to realising such an ideal lamp. When the possibility of obtaining light by electricity first presented itself, the incandescence of refractory materials was at once thought of as the light-giving medium. The choice was not very difficult, for platinum and carbon alone unite the necessary qualities of being conductors of electricity and being capable of resisting high temperatures. But even platinum has to be protected against excessive currents, which would melt it, and the carbon is liable to be burnt, when exposed to the oxygen of the air, while it is heated by the current. We find, therefore, that De Moleyns in 1841, King in 1845, and Staite in 1848, propose to enclose the incandescent material in glass globes, from which the air is exhausted. Such incandescent lamps evidently promise to fulfil all that can be desired, but their construction presents very great difficulties, as a total absence of oxygen is necessary, if the gradual destruction of the electrode is to be avoided. Another form of incandescent lamps was brought to the public notice about sixteen months ago by Mr. Reynier and by Mr. Werdermann, who both employ an incandescent carbon rod, which is pushed against a carbon disc by a mechanical contrivance. In the newest form of his lamp Mr. Werdermann employs a *copper* disc and claims the advantage for it, that it does not waste away. By these lamps a very steady light is given out and the whole construction is very simple and handy, but for a given power, expended on the machine producing the current, much less light is obtained by incandescence

than by an arc, and the question of cost makes them only applicable under certain favourable conditions.

A second method of making a simple lamp is shown by the construction of the Jablochkoff candle, and the facility with which the whole apparatus is managed, captivated at once the general attention, when it was first brought out. The successful application of these candles to the lighting of the Avenue de l'Opera in Paris, as already mentioned, has given a great impulse to the introduction of the electric light which was regarded up to that time by the general public, as a scientific toy incapable of practical work. But these candles have some serious drawbacks which have shown themselves during the practical working of the system. When once a candle is extinguished, it cannot be relighted, and all the candles in the same circuit must go out at the same time. A candle will last only for a short time and special arrangements have to be made to switch the current on to a fresh one. This is either effected by a commutator being fixed near each lamp, and an attendant going from lamp to lamp to turn the commutator; or special wires have to be laid to each candle from the machine. In the latter case the fresh candles are brought into circuit by a switch in the engine room, but the first cost of the plant is thereby materially increased.

Various proposals have been made to relight candles automatically, and to bring the fresh candles into circuit by mechanical contrivances. But by destroying the simplicity of the candle its chief advantage disappears, and the management of those improved forms requires considerable skill and knowledge. A minor disadvantage of all candles, where two carbons are placed parallel to each other, is, that the light is not evenly distributed all round. By the use of opal globes this fault is, however, to a great extent counterbalanced, but at the same time much light is in that way sacrificed.

The lamps, in which the carbon rods forming the electrodes are presented to each other end to end, are by far the most numerous, and they have been brought to a high state of utility, although they are of necessity more complicated than either incandescent lamps or candles.

As these lamps produce the light by maintaining a voltaic arc, the current is better utilized in them than in the incandescent lamps, and they have this advantage over the candles, that they can be more easily arranged to burn a long time.

The first lamps of this type were fitted with clockwork, moved by springs, and controlled by electro-magnets, and were in consequence very complicated and clumsy.

In course of time the weight of the carbon-holders was made to move the clockwork, and the latter has become simpler and simpler with every new modification of lamp proposed.

This change from complication to simplicity is very well illustrated by the lamps on the table, constructed by Messrs. Siemens Brothers, and the steps from one type to the other are sufficiently interesting to justify a detailed description.

The first is the medium-size clockwork lamp, invented by Mr. v. Hefner-Alteneck, chief constructor for Messrs. Siemens and Halske, of Berlin. Its action may be explained in this way: The weight of the upper carbon-holder and its carbon imparts motion to the wheels, and lifts the lower carbon-holder until the carbons touch, and the current begins to flow. On its way to the carbons the current has to pass an electro-magnet, and sets the armature of the same in a vibrating motion, and this turning a ratchet wheel, by means of a lever and a pawl, separates the carbons until the current is so far weakened that it can no longer move the armature. Means are provided for moving either the lower carbon-holder alone, or both carbon-holders at the same time, and the result is that the lamp does its work extremely well, but it requires an amount of skill and practice in setting it to work that is at times exasperating.

The next form of lamp, also an invention of Mr. v. Hefner, recommends itself at once by the almost total absence of wheels and the simplicity of its moving parts. The lower carbon-holder is in this lamp a fixture, and the upper carbon-holder is formed by a rack, which in sinking down will turn a pinion. In order to moderate the speed with which this pinion turns, a common escapement-wheel with its pendulum is fixed to the same axle. A movable frame, serving as a guide to the upper carbon-holder,

carries the pinion and the pendulum, being lifted, more or less, by a *solenoid* acting on an iron core connected to the framing. During the normal burning of the lamp, a small lever fixed to the movable frame catches the pendulum, preventing it from moving, and thus keeping the upper carbon-holder stationary. When the arc becomes too large, or the current is weakened by other causes, the *solenoid* will let the frame drop a certain distance, the free end of the little lever is arrested by a projection of the lamp casing and the pendulum is free to move. The upper carbon will then at once descend, but as soon as the distance between the carbons is diminished, the strength of current will increase, lift the frame, and the little lever will again stop the downward motion of the upper carbon-holder.

In order to lessen the suddenness of the motion of the framing, an air-pump is connected with it, and a spiral spring is attached to the core, by which the attractive force of the solenoid can be more or less assisted according to the strength of the current. In practical work this form of lamp has proved to be very efficient, as its management is easily understood, and hardly any part of it can get out of order. Six such lamps have been in use at Blackpool, a watering-place in Lancashire, during two months in all sorts of weather, and never failed after a few mechanical imperfections had been removed. Similar lamps are at work in the British Museum, where all the apparatus has been managed, after the first fortnight, by the Museum authorities themselves, and no difficulty has been experienced by them in maintaining the regulators in good working order. At present these lamps are being exchanged for others which work on the same principle, but have the case containing the solenoid and the moving frame above the point of light. This modification has been adopted because it facilitates the construction of suitable lanterns, but it does not differ from the form first described in the way of regulating the approach of the carbons.

In these lamps, as in most of those of other makers, the strength of current regulates the distance of the carbons, and the consequence is, that it is not possible to connect two or more of them in one circuit. To overcome this difficulty, Mr. v. Hefner used

another principle, illustrated by the third lamp on the table, which in some respects resembles the pendulum lamp. The upper carbon is attached to a similar rack moving in a slide, and turning a pinion with pendulum attached, but the motion of the movable frame is governed by *two* solenoids instead of one. The frame is attached to a lever, which carries a double iron core, reaching into the two solenoids. One of these acts in the same way as the solenoid of the pendulum lamp, separating the carbons whenever a current passes through it. The other one consists of fine wire, having a high resistance and forms a shunt to the main circuit, the ends of the fine wire being connected direct to the terminals of the lamp, and by attracting its core it brings the carbons together or releases the pendulum respectively. The action of these solenoids will, therefore, be balanced when the difference of potential on the two sides of the arc is of a certain magnitude, depending on the relative position of the two coils and the resistance of the wire on them. By this arrangement the quantity of the current flowing through the lamp has no influence on the relative position of the carbons, and nothing prevents a large number of them being inserted into one circuit. In producing light by alternate currents as many as 24 of these lamps have been worked in series, and their behaviour was all that could be desired. In order to make these lamps independent of each other a little contact piece is attached to the movable frame, which makes a short circuit from one terminal to the other whenever the frame is in its lowest position.

Both in the "pendulum" and in the "differential" lamp the lower carbon is fixed, the focus of the light will therefore gradually descend. For some purposes it is, however, necessary to keep the focus in the same place, and Dr. William Siemens has suggested a simple contrivance to attain this end. The lower carbon is enclosed in a tube and, by means of a fine wire, a roller and a weight pushed against a screw fixed to the upper end of the tube. As the carbon wastes away by the action of the current fresh carbon is fed upwards by the weight, and the shape which the carbon assumes admits of the screw being far enough away from the arc to prevent its being injuriously affected by the heat. It is obvious that in such a case much longer carbons can be used,

and that the time, during which a lamp can remain alight without removal of carbons, is thereby very materially increased.

This "abutment" pole is employed for both electrodes in the last form of lamp, invented by Dr. William Siemens, but the screw, against which the carbons are pressed, has been replaced by a knife-edge, which appears to give better results. In this lamp the carbons are placed horizontally, and their tubes are attached to bell crank-levers, the other ends of which support the core of a solenoid, on which fine wire is wound, forming a high resistance shunt from one terminal to the other. The action of the lamp is very simple; the weight of the core, which can be varied at will, keeps the carbons apart when no current passes. As soon as a current arrives the solenoid will lift the core, the carbons touch for a moment and the arc is established, the further regulation depending again on the difference of potential *only*, and being independent of the *strength* of the current. It will be noticed that no wheels whatever enter into the construction of this lamp, and that all its parts are exceedingly simple.

There is no doubt that other makers have made a similar progress in the construction of their lamps, but, as stated above, their variety is so great, that the task of describing them can hardly be undertaken by any one person alone, but especially by the author, whose intimate connection with one system makes him somewhat blind to the advantages of others.

A further requisite for the production of a good electric light are suitable electrodes, and at present it seems settled that carbon is the right material for the purpose. Various plans have been suggested to arrest or retard the wasting of the carbons, but none of them has as yet undergone a practical test, and nearly all of them have the disadvantage of complicating the construction of the lamps. On the other hand, great attention has been paid to the manufacture of carbons, and their quality has consequently much improved since larger quantities of them have been consumed.

It can, therefore, safely be said, that the apparatus necessary for the production of the electric light has at present reached a sufficiently advanced stage to make its general introduction possible.

As soon, however, as the general public is invited to make use of the electric light for ordinary lighting purposes, the question of *cost* decides its adoption or rejection in every particular case.

A short calculation will show that a strong light, produced by a continuous current, is very much cheaper than a number of alternate current lights, which in their turn are more economical than gas lights, provided that in all three cases the same amount of light is generated. In making the comparison, it is assumed that a 100 candle Sugg gas-burner will consume 23 cubic feet of gas per hour, costing 3s. 6d. per 1,000 cubic feet: further, that a 400 candle alternate current light requires ½ horse-power, and that it consumes 3 inches of carbon per hour, costing 4½d. per foot; and that a 6,000 candle continuous current light requires 4 horse-power, consuming 3 inches of carbon per hour, costing 8d per foot.

When the electrical machines are driven by a gas engine consuming 26 cubic feet of gas per hour per horse-power, the relative cost of maintaining a light of 6,000 candle power is as follows:—

| | £ | s. | d. |
|---|---|---|---|
| *For gas* ... ... ... ... ... | 0 | 4 | 10 |

For *alternate current electric lights* (15—400 candle lights):

| | s. | d. |
|---|---|---|
| 200 cubic feet of gas for the motor ... ... | 0 | 8½ |
| 3 feet 9 inches of carbon, at 4½d. per foot | 1 | 4½ |
| Attendance, &c. ... ... ... ... | 0 | 6 |
| Total cost ... | 2 | 7 |

showing a saving of 47 *per cent. over gas.*

For *continuous current* light:—

| | s. | d. |
|---|---|---|
| 114 cubic feet of gas for the motor ... | 0 | 4¼ |
| 3 inches of carbon, at 8d. per foot... ... | 0 | 2 |
| Attendance, &c. ... ... ... ... | 0 | 1¼ |
| Total cost ... | 0 | 7½ |

showing a saving of 87 *per cent. over gas.*

When steam engines are employed, an even greater saving can be shown in favour of the electric light, the cost being reduced to 2s. 1d., and 5d., respectively; assuming that 4 lbs. of coal, costing 15s. per ton, will produce 1 horse-power per hour.

Although these figures most certainly indicate that a great future is before the electric light, a careful consideration of the data, on which they are based, will demonstrate that the value of gas shares need not be affected by the introduction of the electric light.

To begin with, the calculation is based upon the comparison of strong lights, for the production of which a continuous electrical current is particularly well adapted, and the capital outlay on gas-fittings, and on electric light apparatus respectively, has not been brought into the account. In practical life, however, the application of such strong lights is very limited, and the capital outlay on the plant for an electric illumination is a matter of serious consideration, because in most cases the outlay for the necessary gas-fittings has already been incurred.

The smaller electric lights are comparatively more expensive, than the large ones, but even the alternate current lights show a considerable saving over the gas lights, and it is quite certain that for illuminating large spaces like concert rooms, stations, and workshops, the electric light will soon supersede gas light.

A few examples of the actual cost of electric light applied to such purposes, will demonstrate its cheapness:—

At the Albert Hall a saving in gas is effected of 25,000 cubic feet per night, or £4 7s. 6d., while the five electric lights cost £1 10s. 6d. for fuel, attendance, and carbons. In this case a pumping engine is used for driving the machinery, which consumes a very large quantity of fuel, and nevertheless a saving of 66 per cent. is effected.

At Blackpool the cost of a five hours' run has been stated by Mr. Chew, who has the apparatus under his management, to be £3 6s. 10d. for six lights of 6,000 candle power each, and an equal amount of light produced by gas would have cost £7 5s. The cost of the electric light is therefore in this case 54 per cent. less,

although a heavy interest on capital outlay has been charged against the electric light, and no such interest is taken into calculation in respect of the gas.

At the British Museum the electric lights were used during 360½ hours between the 28th October, 1879, and the end of February, thus giving to the readers during the eighteen weeks, fully nine weeks of artificial light, and thereby just doubling the time during which they could pursue their duties. The motive power for the electrical machines is supplied by two 8 horse-power semi-portable steam engines made by Messrs. Wallis and Stevens, of Basingstoke, and specially fitted for economical and regular working. In the Reading Room itself four lights are suspended, being each of about 4,000 candle power, and in the halls and in front of the Museum seven 400 candle lights are placed. The necessary currents are generated by *four continuous* current machines for the *Reading Room* and by *one alternate* current machine for the *other* lights, a fifth medium machine serving as exciter for the field magnets of the alternate current machines and the four light machines. It was found, that the machines working up to their full power, produced too much light: the speed has been, therefore, reduced until each light gives only 4,000 candles as just stated. The mode of working the lights is to burn them for a short time in the morning, after new carbons have been put in, to ascertain that everything is in order, and to keep the fires banked up during the day, so that the lights can be started at ten minutes' notice, should a sudden fog come on. For this reason the consumption of fuel is a little higher than might be expected: the cost of carbons is also considerably more than the general estimate given above, because a special sort of carbons is employed, which are manufactured in Germany and are much more expensive than those made here. The amounts for fuel and for carbon include also the cost of fuel and carbon used during the week's trial, before the Reading Room was opened to the public, and the stock of carbons at present in the Museum, which will be sufficient to last until the end of March.

Mr. Bond, the principal Librarian, has kindly permitted the

statement that the money paid by the Museum authorities for the 360 hours of electric lighting amounts to—

| | £ | s. | d. |
|---|---|---|---|
| Carbons ... ... ... ... ... | 50 | 15 | 10 |
| 23 Tons of Coal at 15s. ... ... ... | 17 | 5 | 0 |
| 18 Gallons of Oil at 4s. 6d. ... ... ... | 4 | 1 | 0 |
| 54 lbs. of Waste at 6d. ... ... ... | 1 | 7 | 0 |
| 2 Sets of Brushes at 5s. ... ... ... | 0 | 10 | 0 |
| 1 Set of Commutator Plates ... ... ... | 0 | 17 | 6 |
| Engine Driver, 18 weeks at 37s. per week ... | 33 | 6 | 0 |
| Total cost ... | £108 | 1 | 4 |

This gives as cost per hour—

| | s. | d. | |
|---|---|---|---|
| For Carbons ... ... | 2 | 9 | |
| Other charges ... | 3 | 3 | |
| Total ... ... | 6 | 0 | for a light of 18,800 candles, |

which amount of light produced by gas would cost at least 15s. per hour, the saving effected being 60 per cent.

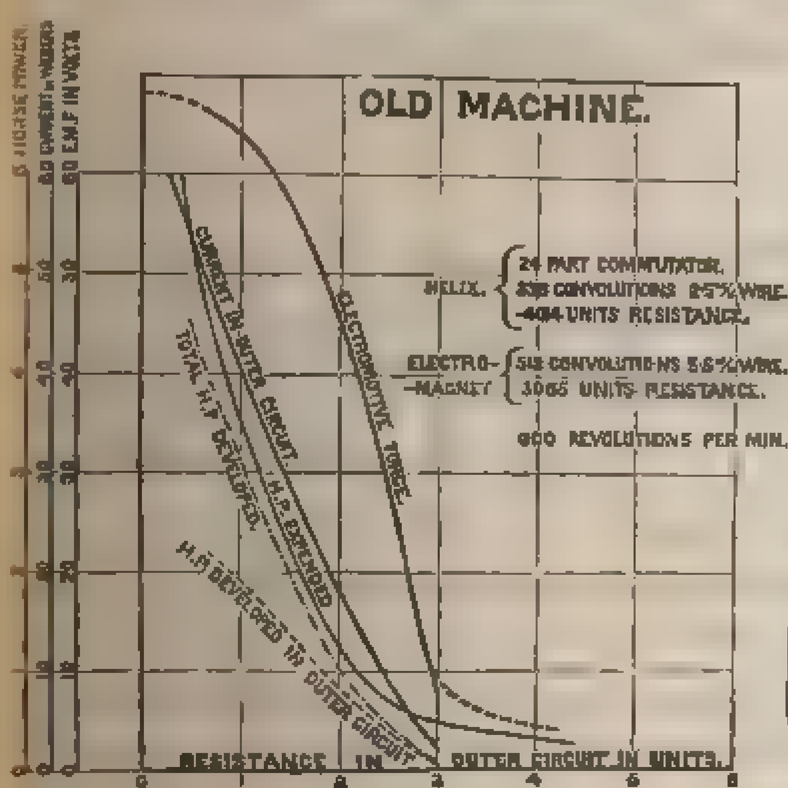
It has been said that an unsurmountable obstacle to the introduction of the electric light was the impossibility of storing electricity in the way in which gas is stored at the gas-works. In using such an argument the fact has been lost sight of that, although electricity itself cannot be collected, motive power can easily be stored, and the latter can be instantly converted into a light-giving current by the aid of the dynamo—or magneto—electric machines.

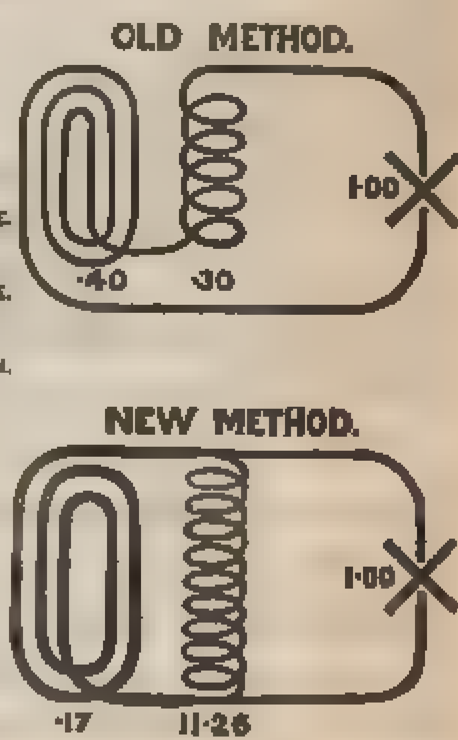
There is no doubt, therefore, that the electric light has left the experimental stage and is now commencing its practical career, and that it bids fair to take rank soon with many of those other modern applications of science which have so rapidly become familiar to us that we scarcely realise the fact that few of the most important of them date further back than the beginning of this century.

The PRESIDENT: Gentlemen,—It is customary at our meetings, before commencing discussion upon a paper, to propose a vote of thanks to the author of it. I am quite sure, from the very hearty applause you gave at the conclusion of the paper, that the usual formality can be dispensed with, and that I may at once assure Mr. Alexander Siemens that we are extremely obliged to him for the very able paper he has brought before us.

We are also much indebted to Messrs. Siemens Brothers, who have allowed their apparatus to be exhibited and used in illustration of the paper, and for the array of facts upon which alone all future opinions of the electric light must be based. The true value of the paper is in the facts it contains, and those facts show that the electric light has a most remarkable future before it. Mr. Siemens will now supplement his paper by a verbal explana- of the diagrams on the board and of the lamps upon the table.

Mr. ALEXANDER SIEMENS: The first diagram ("new method") shows Wheatstone's method of using the generated current. As the current passes from the helix it is divided into two branches, the one portion passing through the high resistance of the electro-

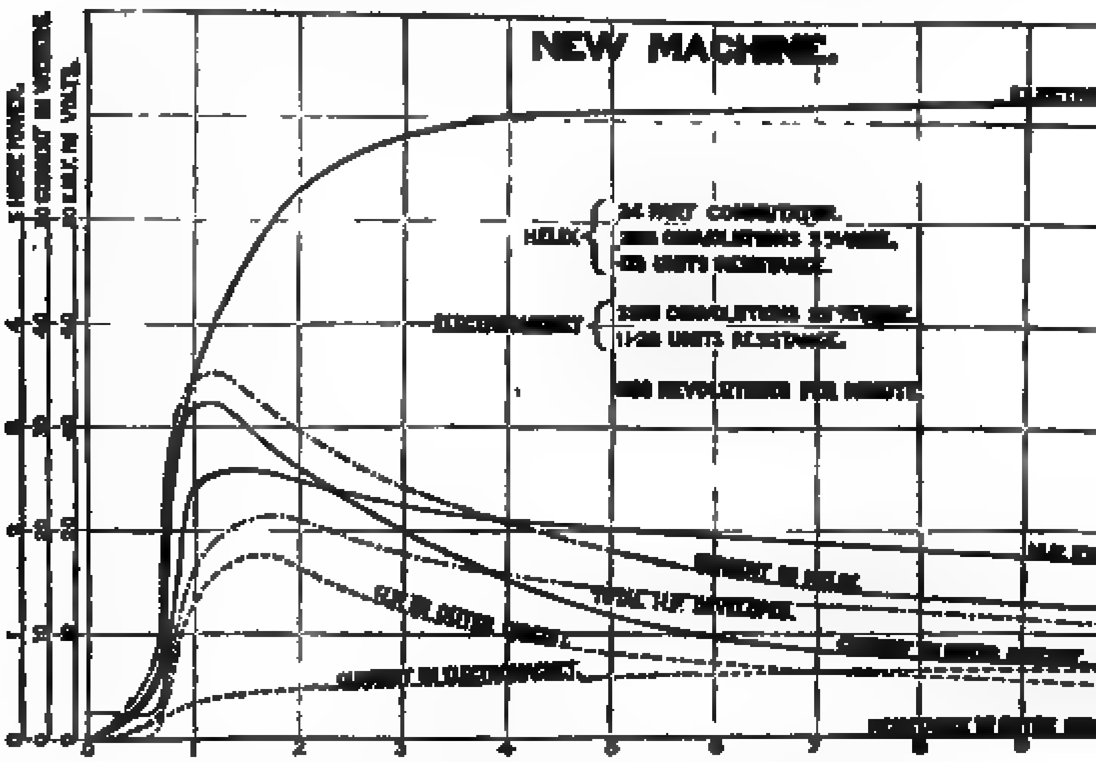



magnets, and the other portion going to the outer circuit, producing the electric light. In the ordinary dynamo machine ("old method"), the current generated in the helix goes through the

electro-magnets to the outer circuit to produce the light, r
to the helix.

Experiments were made in the works of Messrs. Siem
the diagram shows the resistance, in ohms, in the outer
The ordinates to the continuous curve with dotted ends r
the electro-motive force corresponding with the circuit re
one black line represents the current in the outer circ
other black line the horse-power expended; and the dot
the total horse-power developed in the outer circuit. It
seen that in this machine the electro-motive force very
diminishes (becoming after a time more steady), but it go
to 10 volts with a resistance of 3 units, and, in conseque
current obtained from a dynamo machine under such c
is not sufficiently strong to produce a good light.




The second diagram shows a very different effect fr
first one. The horizontal line represents the resistance in th
circuit, the upper curve the electro-motive force, which,
be seen, rises rapidly and becomes constant after the resis
greater than 4 units. The current in the outer circuit r
the outer resistance is about 1½ units, when it falls much
than in the ordinary dynamo machine. The method of
the machine in this way makes it possible to get a fair cur
giving a light with much greater resistance in circuit.

The lamps on the table were brought before the meeting to show the change from the clockwork arrangement to the simple lamp in which all wheels are dispensed with.

In the old clockwork lamps, the weight of the descending upper carbon-holder turns the wheels and lifts the lower carbon-holder until the carbons touch; a current will then flow through the lamp and reverse the motion of the wheels by imparting a vibrating motion to the armature. As stated in the paper, it is a good lamp, and capable of giving excellent results, but its delicate construction makes it difficult to manage.

In the second, or pendulum lamp, the current passes through a solenoid, giving an up and down motion to the frame. The upper carbon-holder is movable, the lower one being fixed. When the current is strong enough to lift the frame, the upper carbon-holder does not move, but, on the frame lowering, a small lever is lifted up, allowing a pendulum to move, which regulates the descent of the upper carbon-holder, being connected to it by a pinion and an escapement wheel. On the carbons coming in contact, the current is strong enough to again lift the frame, when the carbons remain stationary till their consumption weakens the current, when the operation of the clockwork is repeated.

In order to convert these lamps into focus-keeping lamps, the following arrangement has been proposed by Dr. Siemens:—The lower carbon is moved upwards by a weight. It is well known that in its combustion the carbon assumes a conical shape. A screw is fixed so as to rest at the base of this cone, and resists the upward tendency of the carbon till the cone becomes reduced, when the carbon is moved upwards till again arrested by the screw, and by this simple action the carbons are kept at a uniform distance. This permanent focus allows carbons of any length to be used.

Here is a differential lamp somewhat similar in construction to the pendulum arrangement I have described. The current acts on a movable frame, the pendulum is released, and the upper carbon-holder allowed to descend. But the motion of the vibrating frame is regulated by two coils, and the current is divided, one part going from one terminal partly through the

solenoid, and through the carbons to the other terminal; the other part proceeding from the same first terminal through the other solenoid, and direct to the other terminal. When the lamp is at work, and the carbons are apart, their separation prevents any current passing through the lower circuit. When the carbons touch, the lower circuit will get almost all the current, and, having less resistance in its circuit, it will attract the armature, and the position of the armature will depend upon the resistance in each circuit. The resistance of the lower circuit is that of the coil and the arc; that of the upper circuit depends upon the amount of wire in the coil. Should, therefore, at any time an extra strong current arrive, it will make no difference in the regulation; for the proportion of the division of the current again depends upon the resistance, *i.e.*, the power of the solenoids will increase in the same proportion as before, and the armature will not move at all. But if the carbons burn away, and the resistance of the arc becomes increased, then the lower solenoid will get less current, and the upper solenoid will attract the frame and bring the carbon again in motion; and by bringing this difference of potential into play any number of lamps can be put in the same circuit.

On the table is a lamp on the foregoing principle, adapted for being placed in the saloon of a ship, being supported by a bracket fixed to the side.

It will be seen that the principle of this last lamp is very simple. There is no clockwork in it whatever, and no fine fittings are required. Its construction meets the requirement that lamps should be as cheap as possible, and its easy management makes it accessible to everybody.

Mr. Crompton: Mr. A. Siemens has referred to the difficulties not yet overcome of providing a steady and continuous feed to the carbons of electric lamps using the arc.

I have been working in this direction for nearly two years. I think that all the feed mechanisms that have been designed are in a sense analogous to the governors of steam engines. Substitute the varying intensity of the magnetic field in the electro-magnets or solenoids of the lamps for the varying centrifugal force of the steam engine governor; again, substitute the mechanism which

controls the advance of the carbons for that which controls the throttle valve or cut-off gear of the steam engine, and the parallel is complete. The efficiency of a steam engine governor depends on its promptness in responding to the slight variations of centrifugal force of revolving masses of metal: the promptness is insured by reducing the mass of the parts actuated as far as possible,—in other words, by giving the centrifugal force but trifling weights to start from one position of stability, and the same trifling weights to be arrested in a new position of stability. Any increase in the weights of the parts, however well and carefully counterbalanced, causes sluggishness in starting, and liability to overshoot the position of stability before the motion of the parts is arrested. This causes a swinging, or, as it is called by enginemen, a "hunting" action of the governor.

Carrying out my parallel further, I contend that all electric lamps *hunt* more or less. They do not commence to feed until the current is very much reduced, and do not arrest the feed until the current is too much increased by the carbons being approached too closely.

Having this in mind, I designed the lamps which your President has called on me to describe.

My lamps have an electro-magnet, as is usual, but I divide the armature of this magnet into two parts; or in other words, I mount a light secondary armature on the back of the large one. The large armature does the work of separating the carbons, and the small one does the delicate work of feeding them together.

The large armature, A, is a flat bar of soft iron, attached firmly to the tube, which forms part of, and which is, in fact, the supporting holder of the negative carbon.

When the current is switched on and passes round the electro-magnet, the armature is attracted down and held down to a fixed stop during the whole time that any current passes. When the current ceases altogether to pass, as may happen through a violent gust of wind or piece of carbon splintering off, the armature is released, and, being supported by a spiral spring, is raised up until it reaches its original position, or the lower carbon touches the upper one, and the current is thus re-established.

The small variations in the strength of the current, which I rely upon to actuate the feed of the upper carbon, are responded to by a very light plate of soft iron, or secondary armature, $a$, hinged to the large armature, and suspended at some distance above it by a very light spiral spring. The tension of this spring can be varied by the regulating screw of the lamp. The secondary armature carries a curved arm, which is applied as a brake to brake-wheel, which is the last wheel of a clockwork train set in motion by the weights of the positive carbon rod, as is usual in these lamps.

The parts moved by the varying magnetic force induced in the armature $A$ only weigh a few grains, and the plate $a$ is so sensitive to it that it is quite usual to see it rise and fall 8 or 10 times during one revolution of the valve wheel,—in other words the feed advances the carbon from 50 to 100 times during the time that one millimetre is being consumed off the two carbons; the feed is therefore practically continuous, and the light proportionately steady.

I join issue with Mr. Siemens as to the advantage of doing away with clockwork. It is a mere question of whether a complicated system of half-a-dozen levers and ratchets, pendulums and catches, is preferable as a mechanical arrangement to four gearing wheels and pinions. If it is, the practice of us mechanical engineers in our tools (as regards the use of gearing) must be far from correct.

We are indebted to Mr. Siemens for calling attention to the extreme importance of external resistance in steadying the currents of dynamo machines. No one who has not had the carrying out of instalments of electric lighting can appreciate the luxury of working through long lengths of cable. Both the machines and the lamps are far easier to manage, and the light is incomparably steadier. Curiously enough, these good effects do not seem to come from mere external resistance only, as an external resistance composed of a small quantity of small wire does not give nearly such favourable results as a great length of insulated cable having a thick conductor does. In the latter case, it appears as if the great surface of conductor and its insulated covering acted in some way as a condenser or fly wheel to the current. Another point

which engages our serious attention is the question whether earth returns can be profitably used for these heavy currents, and, if so, what precautions must be taken?

I shall be glad to hear Dr. Siemens' experiences on this subject: for my own part, I have never yet been successful in working with an earth return.

I have found the resistance to the passage of lightning currents so variable as to endanger the safety of the bobbins of the machine, from suddenly falling from, say, 1·5 ohms to zero.

I think the lamp described and shown by Mr. Siemens, in which a shunt current is utilised to work the feed of the carbons, is a step in the right direction. Several others, myself amongst them, have been working in this direction, and I hope soon to show you my own lamp made on this principle. This principle will enable the prime cost of cables to be kept down very considerably, as we shall, I hope, be able to burn several arc lamps in series.

I may be mistaken, but I think Mr. Siemens has done his lamp now burning an injustice, by working with the very small internal resistance he has now in circuit. If this had been increased, he would have sacrificed a very small percentage of efficiency, but gained greatly in steadiness of current and light.

The PRESIDENT: We will put the current on to Mr. Crompton's lamp, that it may have a trial.*

Mr. CROMPTON: My lamp has not been on a Siemens' machine before, and I am pleased to see it burn so satisfactorily. Six of these lamps are now being fitted up for St. Enoch's Station at Glasgow. They burn for nine hours without change of carbons. The lamp is necessarily tall, on account of having to provide for burning of 27 inches of carbon. The only way the carbon could be got into a smaller space would be by adopting Mr. Heinrichs' ring form of rods.

Mr. HENRICHS: I have pleasure in introducing to you my new lamp. Experience with the electric light has proved to me that

* The hall, which had been previously illuminated with a Siemens' electric light, was now lighted by a Crompton's lamp.

neither difficult mechanism nor complicated arrangements for producing and regulating the light are required, the only desiderata being good currents and good carbons.

In Mr. Siemens' lamp the carbons are straight, and placed vertically above each other; but my carbons are made in the form of a ring cut in two, thus enabling a long carbon to be brought into use. The advantages of my lamp are, first, that the mechanism for regulating the light is above the arc, and consequently there is no shadow: secondly, two magnets, through which the current passes, are used—one to separate the carbons when the arc is started, and the other to work a step-by-step arrangement to regulate the distance as the current fluctuates in strength.

The lamp before you has previously burned for thirty-six hours successively with a Silvertown machine. The carbons are of high resistance, not being sufficiently compressed, and with the machine now working the arc is not properly maintained. This machine has a low internal resistance. The current supplied is therefore not suitable to my carbons, and a trial to burn the lamp will not be successful.

In a few months I hope to be able to show you, under more favourable conditions, my new lamp, without clockwork or mechanism for regulating the feed of the carbons.

(An attempt was made to set the lamp going, but the experiment was only partially successful.)

The President then called upon Mr. André to explain his incandescent lamp with Mr. Brougham's improvement.

Mr. André: I regret that one of my lamps with Mr. Brougham's water-covered joint is not on exhibition before the meeting. In this lamp, the carbons are enclosed in an atmosphere rendered incapable of supporting combustion by the action of the incandescent carbon itself. The lamp is enclosed in an air-tight manner, so that when all the contained oxygen has been taken up by the burning carbon, there remains an atmosphere of nitrogen and carbonic oxide only. In this atmosphere combustion ceases, and the carbons are but slowly roasted by disintegration. Mr. Brougham's improvement consists in covering the joints with a liquid, by which means air is

tainly excluded from the lamp. This is important, for the ltration of air not only consumes the carbon, but, if it takes ice when the lamp is not burning, it causes the atmosphere within lamp to become inflammable.

The liquid is also of use in diffusing and softening the light ien placed around the glass of the lamp; good effects may be tained by colouring the liquid.

In this lamp, the rate of consumption of the carbon is slow, and the quality of current required is small, subdivision may be rried out to a great extent. At Stafford House, we had thirty-six mps on one small (A size) Gramme machine.

Mr. ANDREWS: My lamps consist of carbon plates placed face face. One form of lamp has three carbon plates—a centre one, Fig. 1, $\frac{1}{32}''$ thick, and two outer ones, *b b*, each $\frac{1}{8}''$ thick, hich are separated from the centre one by four bits of slate, *g g*, aced at the bottom corners of the plates, leaving an air space of $''$ between the pairs of carbon plates. Two stout springs, *s s*, are ranged near the bottoms of the plates, to press the bits of slate id the plates together, and keep the latter separate and parallel. hese springs convey the current to the outer plates, which are the ectrodes. The arc presenting itself at the upper edges burns way the plates downwards like a candle; the centre one consumes t the same rate, and prevents the arc travelling in between. On ccount of the poles being exposed to the atmosphere on one side nly, the upper edges always remain bevelled on that side.

The arrangement for lighting the lamp is made by an electro-nagnet attracting an armature, and taking a small piece of carbon way from the lower part of the side edges of the electrodes, as een in Fig. 2. The small piece of carbon, *c*, is attached to the ipper end of a bell crank lever, *l*, and the armature, *a*, is fixed o the end of the other arm of the lever, under the pole of an lectro-magnet, *m*, which is in the lamp circuit. When the urrent is established, the armature is attracted by the electro-nagnet, and draws away the little piece of carbon, when the arc resents itself, and slowly travels to the top edges of the plates, where it continues until the carbons are consumed. The plates ire cut away obliquely on one side to prevent the arc ascending

too quickly, and cause risk of going out. Alternate currents are used, or continuous currents alternated at periods of about one minute, so that the carbons shall burn away at the same rate. The following are the most striking properties of the lamp. There are no movable parts after it is once lighted. One set of carbons, $6'' \times 4''$, burns for a week of nights of ten hours each, therefore requiring attention only once a week. The resistance of the carbon plates is very small. The light is quite steady.

Another form of lamp (which, however, I have not yet succeeded in perfecting) consists of two discs placed one above the other. The upper disc, *a*, Fig. 3, is about $\frac{3}{8}''$ thick, and about $\frac{3}{4}''$ larger all round than the lower disc, *b*, which is $4''$ diameter and $\frac{1}{8}''$ thick; an insulating material of $\frac{1}{32}''$ thick is placed between the discs. The arc presents itself at the periphery of the lower carbon and under surface of the top carbon, and is caused to rotate by a few convolutions, *c*, of the circuit wire on the stand above the discs. The upper disc is the positive pole; and as the crater of the positive carbon is the greatest source of light, the light is thrown down unobstructed by any shadow from the negative carbon. The current is conveyed to the positive disc by a brass tube, *d*, down the centre of which passes a rod to the negative carbon. The arc being only $\frac{1}{32}''$ long, a great number of lights may be burnt in one circuit.

The PRESIDENT: There are several other improvements before the meeting, and so many gentlemen who are anxious to speak on this subject, that I think I shall meet the wish of the members present by postponing the discussion until our next meeting, by which time intending speakers will no doubt be ready with their remarks. A ballot for new members will now take place.

The result of the ballot showed that the following were elected

*As Foreign Members:*

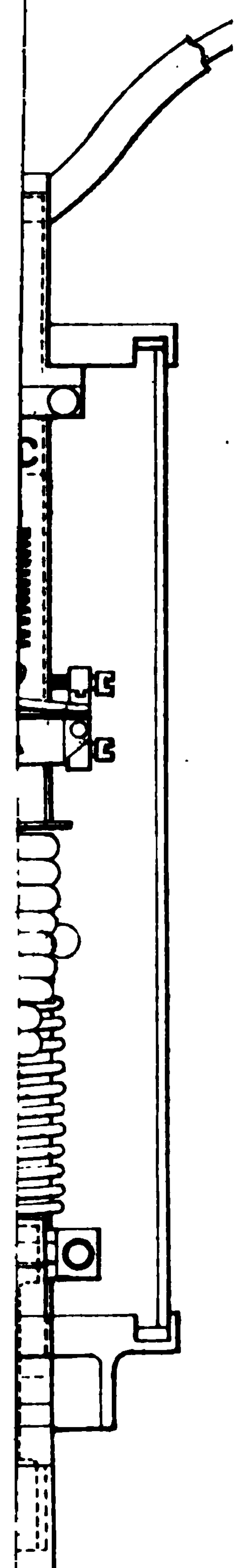
D. Monnier.

Gustave P. Seligmann-Lui.

*As Associate:*

Henry Francis Wilkes.

The meeting then adjourned until Wednesday, March 24th.



# ORIGINAL COMMUNICATIONS.

## ON THE ELECTROSTATIC CAPACITY OF SUSPENDED WIRES.

By OLIVER HEAVISIDE (*Associate.*)

Suppose, in the first place, we have a single wire suspended in empty space, and charged—no matter how—with a quantity $q$ of electricity per unit of length. The resultant force at a point whose perpendicular distance from the centre of the wire at any point A is $r$, due to the elementary charge $q\,d\,x$ at distance $x$ from A is

$$q\,d\,x \div (x^2 + r^2),$$

and this resolved in the direction normal to the wire is

$$q\,d\,x \div (x^2 + r^2) \times r \div (x^2 + r^2)^{\frac{1}{2}};$$

therefore the resultant force due to the whole charge is

$$\int_{-\infty}^{\infty} \frac{q\,r\,d\,x}{(x^2 + r^2)^{\frac{3}{2}}} = \frac{2\,q}{r}.$$

Since the resultant force is the rate of decrease of the potential, the potential is

$$V = 2\,q \log. \frac{z}{r}$$

where $z$ is a constant. If the potential at an infinite distance is zero or constant, the potential of the wire itself is infinite; or, in other words, an infinite amount of work must be done to charge the wire—that is, it would be impossible to charge it. This may be made more intelligible in another way. The capacity of a wire becomes smaller and smaller the further it is removed from other conductors, and in the limit, when the wire is alone in space, it vanishes.

Suppose, now, there is another wire parallel to the first at

distance $2h$, and charged with $-q$ per unit of length; the potential due to its charge at distance $r'$ is

$$-2q\log.\frac{z'}{r'}$$

where $z'$ is another constant; consequently the potential due to both charges is

$$2q\log.\frac{r'}{r}$$

for $z$ and $z'$ both disappear on being made infinite. Therefore, if $d_1$ and $d_2$ are the diameters of the wires, their potentials are

$$2q\log.\frac{4h}{d}\quad\text{and } 2q\log.\frac{d_2}{4h}$$

Thus the charge divided by the difference of potentials is

$$q \div \left(2q\log.\frac{16h^2}{d_1 d_2}\right) = \frac{1}{2\log.\frac{16h^2}{d_1 d_2}},$$

and this is the mutual capacity per unit of length of the two wires in space.

The potential is zero at all points where $r = r'$, that is, in a plane equidistant from the two wires, whose shortest distance from them is $h$; and the difference of potential between either wire and this plane is half that between the two wires. It follows that the capacity of a wire of diameter $d$ suspended *alone* at height $h$ above the ground is

$$c = \frac{1}{2\log.\frac{4h}{d}}$$

per unit of length, in electrostatic measure. (F. Jenkin, *Electricity and Magnetism*, p. 332.)

If $h = 3$ metres and $d = 4$ millimetres, $c = \cdot0624$. To bring into electromagnetic measure this must be divided by $(28\cdot8 \times 10^9)^2$; to bring the result into microfarads, multiply by $10^{15}$; and lastly, multiply by the number of centimetres in a mile to find the capacity in microfarads per mile. The result is $\cdot0121$ microfarads per mile.

Next, suppose the line consists of two wires, 1 and 2, of radii

$r_{11}$ and $r_{22}$. Let $r_{12}$ = distance between the centres of 1 and 2, $s_{11}$ the distance between the centres of 1 and of its image, $s_{12}$ the distance between the centres of 1 and the image of 2, or of 2 and the image of 1, and $s_{22}$ the distance between the centres of 2 and its image. Also let $V_1$ and $V_2$ be the potentials of 1 and 2, and $q_1$, $q_2$, their charges per unit of length. Then

$$V_1 = 2\,q_1 \log.\frac{s_{11}}{r_{11}} + 2\,q_2 \log.\frac{s_{12}}{r_{12}}$$
$$V_2 = 2\,q_1 \log.\frac{s_{12}}{r_{12}} + 2\,q_2 \log.\frac{s_{22}}{r_{22}}$$

express the potentials in terms of the charges. For $2q_1, \log.\frac{s_{11}}{r_{11}}$ is the potential of 1 due its own charge and the opposite charge of its image, and $2q_2 \log.\frac{s_{12}}{r_{12}}$ the potential of 1 due to the charge of 2 and the opposite charge of its image, and similarly for $V_2$. From these we deduce

$$q_1 = c_{11}\,V_1 + c_{12}\,V_2$$
$$q_2 = c_{21}\,V_1 + c_{22}\,V_2$$

where

$$c_{11} = \frac{2}{R}\log.\frac{s_{22}}{r_{22}}, \quad -c_{12} = \frac{2}{R}\log.\frac{s_{12}}{r_{12}}, \quad c_{22} = \frac{2}{R}\log.\frac{s_{11}}{r_{11}}$$

and

$$R = 2\log.\frac{s_{11}}{r_{11}}.\ 2\log.\frac{s_{22}}{r_{22}} - \left(2\log.\frac{s_{12}}{r_{12}}\right)^2$$

Here $c_{11}$ is the capacity per unit length of wire 1, $c_{22}$ the capacity of wire 2, and $c_{12}$ the mutual capacity of 1 and 2.

Suppose the wires have the same radius $r$, and their distance apart is $d$, at the same height above the ground. Then $r_{11} = r_{22} = r$; $r_{12} = d$; $s_{11} = s_{22} = 2h$; $s_{12} = \sqrt{d^2 + 4h^2}$; and

$$c_{11} = c_{22} = \left(2\log.\frac{2\,h}{r}\right) \div R; \quad -c_{12} = \left\{2\log.\frac{(d^2 + 4\,h^2)^{\frac{1}{2}}}{d}\right\} \div R$$

where

$$R = \left(2\log.\frac{2\,h^2}{r}\right) - \left(2\log.\frac{\sqrt{d^2 + 4\,h^2}}{d}\right)^2$$

The capacity of each wire is increased by the presence of the

other. If the height, as before, is $h = 3$ metres, the radius $r = \cdot 002$ metre, and the distance apart $d = \cdot 5$ metre, then

$$c_{11} = c_{22} = \cdot 0691, \; - c_{12} = \cdot 0215$$

in electrostatic measure. Or

$$c_{11} = c_{22} = \cdot 0134, \; - c_{12} = \cdot 00417.$$

microfarads per mile.

As the capacity of the single wire of the same radius and at the same height was ·0121 microfarads per mile, the presence of the other wire increases its capacity about 11 per cent. If one of the wires is charged by a battery and the other is to earth, then about $\frac{3}{10}$ths of the opposite charge will be on the second wire and $\frac{7}{10}$ths on the earth.

The formulæ for the capacities of any number of wires may be easily obtained, though the subsequent numerical calculations become complex. Suppose the wires have radii $r_{11}$, $r_{22}$, $r_{33}$, $\cdots$, potentials $V_1$, $V_2$, $V_3$, $\cdots$, and charges $q_1$, $q_2$, $q_3$, $\cdots$ per unit of length. Let the distance between the centres of any two wires $m$ and $n$ be denoted by $r'_{mn}$, and the distance between the centre of any wire $m$ and the image of any wire, $n$ by $s_{mn}$. Then the potentials are expressed in terms of the charges by

$$V_1 = 2\, q_1 \log. \frac{s_{11}}{r_{11}} + 2\, q_2 \log. \frac{s_{12}}{r_{12}} + 2\, q_3 \log. \frac{s_{13}}{r_{13}} + \ldots$$

$$V_2 = 2\, q_1 \log. \frac{s_{21}}{r_{21}} + 2\, q_2 \log. \frac{s_{22}}{r_{22}} + 2\, q_3 \log. \frac{s_{23}}{r_{23}} + \ldots$$

$$V_3 = 2\, q_1 \log. \frac{s_{31}}{r_{31}} + 2\, q_2 \log. \frac{s_{32}}{r_{32}} + 2\, q_3 \log. \frac{s_{33}}{r_{33}} + \ldots$$

To find the capacity of any wire, say 1, with respect to itself and the rest, express $q_1$ in terms of the potentials,

$$q_1 = c_{11}\, V_1 + c_{12}\, V_2 + c_{13}\, V_3 + \ldots$$

Then $c_{11}$ is the capacity of 1, $c_{12}$ the mutual capacity of 1 and 2, and so on.

If there are four wires of the same radius, one pair, 1 and 2, at one height, and the other pair 3 and 4 vertically beneath the first pair at another height, 3 being under 1, and 4 under 2, then we have the following relations amongst the distances:—

$$r_{11} = r_{22} = r_{33} = r_{44};\ r_{12} = r_{34};\ r_{13} = r_{24};\ s_{11} = s_{22};\ s_{33} = s_{44};$$
$$s_{13} = s_{31} = s_{24} = s_{42};\ s_{14} = s_{41} = s_{23} = s_{32}.$$

Let

$$\log.\frac{s_{11}}{r_{11}} = a;\ \log.\frac{s_{12}}{r_{12}} = b;\ \log\frac{s_{13}}{r_{13}} = c;\ \log.\frac{s_{14}}{r_{14}} = d;$$
$$\log.\frac{s_{33}}{r_{11}} = e;\ \log.\frac{s_{34}}{r_{12}} = f.$$

Then

$$c_{11} = c_{22} = \{a(e^2 - f^2) + d(cf - de) + c(df - ce)\} \div \mathrm{R}$$
$$c_{33} = c_{44} = \{e(a^2 - b^2) + d(bc - ad) + c(bd - ac)\} \div \mathrm{R}$$
$$-c_{12} = \{d(df - ce) + c(cf - de) + b(e^2 - f^2)\} \div \mathrm{R}$$
$$-c_{24} = -c_{13} = \{c(c^2 - d^2) + b(de - cf) + a(df - ce)\} \div \mathrm{R}$$
$$-c_{23} = -c_{14} = \{b(df - ce) + a(de - cf) + d(c^2 - d^2)\} \div \mathrm{R}$$
$$-c_{34} = \{d(bd - ac) + c(bc - ad) + f(a^2 - b^2)\} \div \mathrm{R}$$

where

$$\mathrm{R} = (a^2 - b^2)(e^2 - f^2) + (c^2 - d^2)^2 + 2(ac - bd)(df - ce)$$
$$+ 2(ad - bc)(cf - de).$$

These are the whole of the capacity coefficients for the four wires, which may now be numerically calculated.

Let the height of the top pair, 1 and 2, be $3\frac{1}{6}$ metres, of the lower pair, 3 and 4, $2\frac{5}{6}$ metres, and let the horizontal distance from 1 to 2 and from 3 to 4 be ·5 metre. Then

$$\begin{aligned}
a &= \log.\ 3166\dot{\cdot}6 &&= 3\cdot5006023, &\log.\ a &= \cdot5441428\\
b &= \tfrac{1}{2}\log.\ 161\dot{\cdot}4 &&= 1\cdot1040115, &\log.\ b &= \cdot0429695\\
c &= \log.\quad 18 &&= 1\cdot2552725, &\log.\ c &= \cdot0987379\\
d &= \tfrac{1}{2}\log.\ 100\cdot384 &&= 1\cdot0008344, &\log.\ d &= \cdot0003622\\
e &= \log.\ 2833\dot{\cdot}3 &&= 3\cdot4522977, &\log.\ e &= \cdot5381080\\
f &= \tfrac{1}{2}\log\ 129\dot{\cdot}4 &&= 1\cdot0560417, &\log.\ f &= \cdot0236810
\end{aligned}$$

Here common logarithms are used. The results are

$$c_{11} = 15\cdot7863 \div \mathrm{R};\ c_{33} = 15\cdot924 \div \mathrm{R};\ -c_{12} = 2\cdot9876 \div \mathrm{R};$$
$$-c_{13} = 4\cdot1992 \div \mathrm{R};\ -c_{14} = 2\cdot4558 \div \mathrm{R};\ -c_{34} = 2\cdot8517 \div \mathrm{R};$$
$$\mathrm{R} = 88\cdot4668,$$

which must be multiplied by ·4343 for the change of logarithms, making

$$c_{11} = \cdot0775\,;\; c_{33} = \cdot0782\,;\; -c_{12} = \cdot0147\,;\; -c_{13} = \cdot0206\,;$$
$$-c_{14} = \cdot0120\,;\; -c_{34} = \cdot0140$$

in electrostatic measure, which are equivalent to

$$c_{11} = \cdot01503\,;\; c_{33} = \cdot01517\,;\; -c_{12} = \cdot00285\,;\; -c_{13} = \cdot00399\,;$$
$$-c_{14} = \cdot00232\,;\; -c_{34} = \cdot00271$$

microfarads per statute mile.

Suppose one of the top wires, say 1, is charged by a battery, while the remaining three wires are to earth. Then wire 1 will receive a charge = ·01503 microfarads per volt per mile, and wires 2, 3, and 4 will receive from the battery opposite charges proportional to ·00285, ·00399 and ·00232. The sum of the latter being ·00916, and the capacity of the first wire ·01503, it follows that about $\frac{9}{15}$ths of the opposite charges goes to the three wires, and the other $\frac{6}{15}$ths to the surface of the earth.

As there are only four wires considered, it is evident that with a large number of wires the proportion of the opposite charge on the surface of the earth becomes quite small, nearly the whole going to the neighbouring wires.

F. Jenkin (*Electricity and Magnetism*, p. 332) says, there is experimental reason to believe that the actual capacity of a suspended wire is about double the amount, calculated on the supposition of there being no other wires on the same poles, owing to the induction between the wires and the posts and insulating supports; but as the posts only occur at intervals, it seems reasonable to suppose that a great part of the difference is rather due to the neighbourhood of other wires. At any rate, a second wire increases capacity about 11 per cent., and with three more the increase is about 24 per cent. according to the above figures, and a greater number will of course produce still further increase.

## NOTE ON THE WATKIN CHRONOGRAPH.

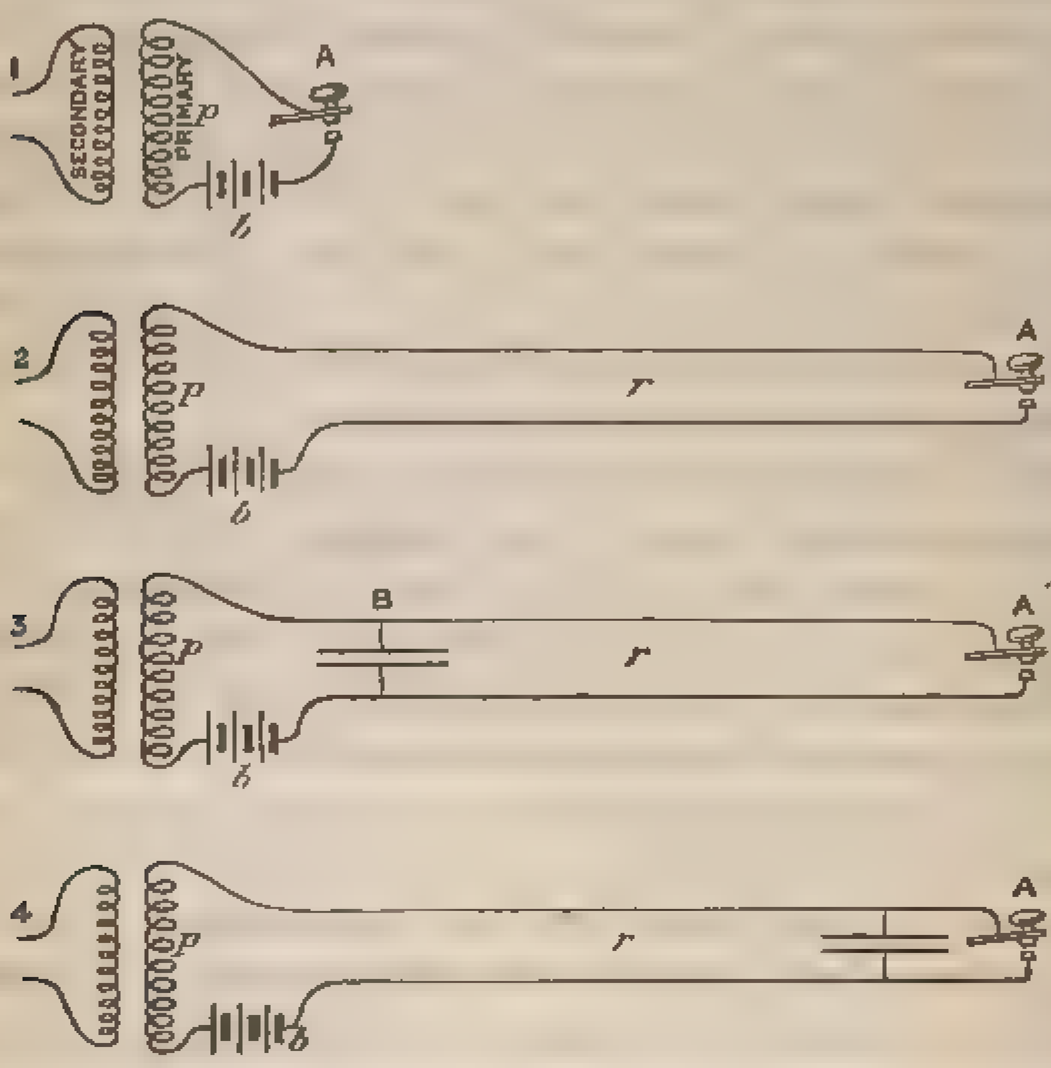
By the EDITOR.

The Watkin Chronograph consists of a weight, which is allowed to fall freely in air between two smoked metallic cylinders. At the moment of firing the gun the circuit of the small electro-magnet supporting the weight is automatically broken and the weight begins to fall. Every time the shot cuts through successive wire screens placed at measured distances apart, the primary circuit of an induction coil is broken, and a secondary spark passes between the falling weight and each of the smoked cylinders burning away a small portion of the lampblack. From the known laws of a falling body, then, the time the shot has taken to go from screen to screen, and consequently the velocity of the shot at different portions of its path can be calculated.

When lending me this apparatus for my recent lecture at the London Institution, Captain Watkin mentioned that it was known that if the screens were far away from the chronograph, it was necessary to use a large induction coil, since a small one failed to produce a spark on the screen being cut by the shot. He had tried to dispense with the large expensive coil, and substitute for it a small one by placing a condenser in the primary circuit; he had found, however, experimentally, that the introduction of such a condenser into the primary circuit at the induction coil itself, or between the induction coil and the screen, produced no improvement, but that, if the condenser were placed in the primary circuit at the screen itself, then that, for some unexplained reason, quite a small induction coil could be used with a screen even quite far away from the coil and chronograph. After thinking over the matter, I sent him the following letter, which he has returned to me for insertion in the Journal :—

"DEAR SIR,—I think the following explains your difficulty regarding the induction coil and the condenser. Let $p$ be the resistance of the primary coil, and $b$ that of the battery employed: then if E is the electromotive force, experiment shows that making

and breaking contact at the point A gives satisfactory secondary currents, that is the sudden production and cessation of a current $\frac{E}{p+b}$ is sufficient for your purpose. Now let a wire of resistance $r$ going to a screen be inserted in circuit (fig. 2), then the current made and broken will only have a strength $\frac{E}{p+b+r}$ which if $r$ is large in comparison with $p+b$ may not be sufficient to produce secondary




currents giving a visible spark at the chronograph. But let a condenser of capacity $c$ be placed at B, figure 3, then when the circuit is open at A there will be an electromotive force E between the plates of the condenser so that it will contain a charge $cE$: when however contact is made at A, the electromotive force between the plates of the condenser will now be reduced to $\frac{rE}{r+p+b}$ and the charge in it to $\frac{rEc}{r+p+b}$, the difference flowing back principally through the wire of resistance $r$ and the contact A. On breaking contact again at A the charge in the

condenser has to be increased again; the first effect, therefore, of breaking contact, when the condenser is inserted, is to suddenly immensely increase the current which directly afterwards falls to nought, producing in this way (if only the capacity of the condenser is large enough) an effect sufficient to balance the diminution of the current produced by putting in the wire of resistance $r$; but if the capacity of the condenser is not large enough it may be necessary to still further increase its effect by putting it at A itself (figure 4): for now when the circuit is closed at A the charge in the condenser is nought, whereas when the circuit is opened at A there is a first rapid rush of a quantity of electricity $cE$ to charge the condenser, followed suddenly by a current nought in the primary coil, and this is sufficient to produce the spark in your induction-coil. Putting, therefore, the condenser at A has the same effect as increasing the capacity of the condenser at B (figure 3); and this is especially the case when the resistance of the extra wire $r$ is large compared with $p + b$.—

"Believe me to remain, very truly yours,

"W. E. Ayrton.

"Captain H. Watkin,

"The Experimental Department, Woolwich Arsenal."

# ABSTRACTS.

## H. MÜLLER—NEW TELEPHONE.

(*Carl's Zeitschrift, B. II.*, 1880, *No.* 3, *pp.* 79–81.)

The peculiarity of this modification of Bell's Telephone consists in a short tube of soft iron being interposed between the usual steel bar and the vibrating plate. This tube is fastened to the vibrating plate, and forms the core of a short coil wound in the opposite way to the coil whose core is the steel magnet. Both coils are in the circuit. The tube has opposite polarity to that of the magnet. A current increasing the magnetism of the steel bar acts also on the iron tube, making the attraction between them much greater; and a current diminishing the magnetism of the steel bar will also act on the iron tube, making the attraction much less, and the vibrations of the tube, and consequently of the plate, under intermittent currents will have greater amplitude than in the simple Bell's Telephone.

## EUGEN OBACH—ELECTRIC PRESSURE REGULATOR.

(*Carl's Zeitschrift, B. II.*, 1880, *No.* 3, *pp.* 69–75.)

It is often necessary in chemical physics to heat substances in a stream of a particular kind of gas for a length of time without altering the pressure of the gas. The author describes the necessity for the automatic instrument described by him, and shown in well-executed drawings accompanying the paper. The arrangement is such that when the mercury in the shorter arm of a siphon barometer is at too low a level it is no longer in contact with a certain wire, and the galvanic circuit is broken, whereby the freed core of an electro-magnet, actuated by a spring, pinches the india-rubber supply tube. When the mercury is at too high a level, contact is made, and the attracted core of the electro-magnet relieves the supply tube.

## C. R. ALDER WRIGHT—ON THE DETERMINATION OF CHEMICAL AFFINITIES IN TERMS OF ELECTRO-MOTIVE FORCE (PART I).

(*Phil. Mag., Vol.* 9, *No.* 56, *April, pp.* 237-266.)

## C. R. ALDER WRIGHT and E. H. RENNIE—DITTO (PART II).

(*Physical Society, February* 28*th*, 1880.)

The first of these papers contains a general sketch of the chief work hitherto done in this direction, commencing with the experiments of Joule, made in 1846, but not published till some years later (*Phil. Soc.*, 1852, vol. iii., p. 481), and the theoretical paper of Sir William Thomson (*ibid.*, 1851, vol. ii.,

p. 429), which served as the first starting point. The second paper contains an account of the authors' experiments, made with the view of determining the electro-motive force corresponding to the affinity between O and H in water, and of demonstrating the existence of sources of error of appreciable magnitude in the experiments made by Joule in 1867 for the British Association, on the determination of the value of J (Joule's constant) by means of the electric current method.

Thomson's demonstration that affinity is expressible in terms of electro-motive force may be put thus: Faraday showed that the weight of a given electrolyte, $w$, decomposed varies as $q$, the quantity of electricity passing, and also as $a$, the equivalent of the compound operated on. Hence $w = a\,q\,\chi$ where $\chi$ is a constant, or at least a multiplier independent of the quantity of electricity passing and of the equivalent of the substance. If $w = a$, $\chi = \frac{1}{q}$, *i.e.*, the value of $\chi$ is the reciprocal of the quantity of electricity requisite to decompose a weight in grammes numerically equal to the equivalent of the substance, or a "gramme-equivalent" of substance. From determinations made by Kohlrausch and others, the value of $\chi$ is found to be 0·000,105 in the C. G. S. system, electro-magnetic measure. The work done in decomposing a gramme-equivalent of substance (*i.e.*, the affinity between the constituents into which it is decomposed) may be expressed accordingly either by H J, where H is a value in heat units, or by $e\,q = \frac{e}{\chi}$, where $e$ is a value in electro-motive force, J and $\frac{1}{\chi}$ being constant multipliers.

When electrolysis takes place the total work done in moving a given quantity of electricity, Q, sufficient to decompose a gramme-equivalent of substance, from one point to another is E Q, where E is the difference of potential subsisting between the points. In practice, part of this work is always done as sensible heat. If the heat thus evolved per gramme-equivalent decomposed be H, the work thus done is H J, so that the work done as chemical decomposition is E Q — H J; this must equal $e$ Q, where $e$ is the electro-motive force corresponding to the affinity between the constituents into which the body is decomposed, whence $e = \mathrm{E} - \frac{\mathrm{H\,J}}{\mathrm{Q}} = \mathrm{E} - \mathrm{H\,J}\,\chi$. Hence the value of $e$ is determinable by finding E and H, that is, by determining in a given experiment, E, the average difference of potential between two given points (*e.g.*, the electrodes of a voltameter), the heat evolved, and the amount of substance decomposed, the value of H being calculable from these two last data.

Joule's observations made in 1846 contain the data for applying this formula, the value of E being calculated from the number of cells of a Daniell's battery used, and some of the tangent galvanometer observations made. The authors find that the values thus obtained for water, copper sulphate, and zinc sulphate are substantially the same as those deduced by Joule from an entirely different mode of reasoning, they have also tried several other methods of determining E, and give the preference to a quadrant electrometer, the scale of which is calibrated by a Clark's cell during the

observations. A series of eighteen experiments thus made give as the final result the value for $e$, in the case of water decomposed into O and H at 15°-20° C., 1·5003 volts. (1·5003 × $10^8$ C. G. S. units), with a probable error of + 0·0048, or + 0·33 per cent. On the other hand, a careful recalculation of all the available data concerning the heat developed when hydrogen and oxygen are burnt to water (the observations being reduced to the standard of atmospheric pressure and 15° 20° C.), gives as final result a development of 34,100 gramme degrees per gramme-equivalent, corresponding to an electromotive force of 1·5038 volts., the value of J being taken as 42 × $10^6$ throughout.

The close agreement between these two values may be taken to indicate that the value of J lies close to 42 × $10^6$. Absolute agreement may be obtained if J be taken as 41·96 × $10^6$, when the value of $e$ becomes from each set of data 1·5023 volts. Certain constant small sources of error in the observations, however, lead to the result that the quantity of substance decomposed is always slightly undervalued, whence the value of $e$ is also slightly undervalued, since the negative term in the expression $e = E - H J \chi$ is overvalued; and hence the ultimate value of J deduced, 41·96 × $10^6$, is undervalued when these sources of error are not taken into account.

The value of J deduced by Joule from his 1867 experiments (a reduction to the standard for a gramme-degree as being the heat requisite to raise a gramme of water from 0 to 1°) represents upwards of 42 × $10^6$, the value varying slightly according as Regnault's formula for the alteration in the specific heat of water with temperature is taken as correct, or Bosscha's formula deduced by recalculation of Regnault's data, but being in each case close to 42·1 × $10^6$. This value, moreover, is shown to be probably appreciably too low, on account of sources of error, the joint effect of which is approximately ascertained to cause a deficiency of near 0·5 per cent. On the other hand, Joule's water-friction values, obtained in 1850 and 1878, accord very closely, and on reduction give numbers very close to 41·5 × $10^6$, so that Joule's value, obtained by the electric current method in 1867, and the value deduced by the authors, both agree so far that they both indicate a value of J considerably more than 1 per cent. higher than that deduced from Joule's most carefully made water-friction experiments. This discrepancy may be explained by supposing that the B A unit of resistance is upwards of 1 per cent. above its theoretical value (1 earth quadrant per second), as such an error would exactly account for the excess in each case. In confirmation of this, Kohlrausch has concluded, from his own redeterminations, that the B A unit is really 1·96 per cent. too large. On the other hand, Lorenz has obtained results (apparently more open to objection than those of Kohlrausch) which indicate that the B A unit is somewhat *below* its theoretical value, whilst Hirn, Violle, and Regnault have obtained values for J higher than 42· × $10^6$, employing methods not involving the B A unit.

The methods adopted by the authors for avoiding, as far as possible, sources of error are fully described, especially a threefold mode of determining the radiation corrections for the calorimeter used (enclosed in a water jacket). The chief source of error above alluded to in Joule's 1867 experiments, lies in

the fact that heat is generated in the experimental wire more rapidly than is possible for it to be communicated to the surrounding water in the calorimeter, and hence that the mean temperature of the wire during an experiment is higher than that assumed to be possessed by it, viz., the mean temperature of the calorimeter, and consequently its resistance is undervalued. On determining the difference of potential set up between the ends of a wire immersed in water, by passing currents of different strengths through it, the water being kept within the same temperature limits in each experiment, so that the mean temperature of the water was practically the same in each case, it was found that the ratio of potential-difference to current, representing the resistance of the wire, was not constant, but rose with the current value. With the apparatus employed, currents of the strength used by Joule (about 0·3 C. G. S. current units, or 3 Webers) caused a superheating of the wire (which had a resistance of a little more than 1 ohm) to the extent of some 8° (calculated from the rise of its resistance) even when the water was kept continually stirred and almost violently agitated throughout to avoid, as far as possible, over-heating of the wire through adherence to it of warmed water. This superheating was increased, as might be expected, by varnishing the wire, or by stirring at intervals instead of incessantly. Sources of error such as these present themselves in all cases where currents of moderate magnitude are passed through resistances for some little time, as in the determination of electro-motive force by methods such as those used by Latimer Clark, *i.e.*, the wire becomes internally heated somewhat above the temperature of the medium surrounding it, and consequently has its resistance proportionately increased by an unknown amount.

## H. BECQUEREL—ATMOSPHERIC POLARIZATION AND THE INFLUENCE OF TERRESTRIAL MAGNETISM ON THE ATMOSPHERE.

(*Journal de Physique, No.* 98, *February*, 1880, *pp.* 51–56.)

The philosophers who, from the time of Arago, have busied themselves with atmospheric polarization have concluded that the plane of polarization of the light sent from any point of the sky passed through the sun, or was perpendicular to a plane passing through that luminary. M. Becquerel was led to think that this coincidence did not generally exist, so that he undertook the problem of determining exactly the relative positions of the sun and of the plane of polarization. After accumulating the results of two years' observations he has come to the conclusion that if we call a plane passing through any point of the sky the eye of the observer, and the centre of the sun *the sun's plane*, then that there is a variable angle between that plane and the plane of polarization of the light coming from the particular point looked at in the sky, such that the plane of polarization is always below the sun, that is, between it and the horizon. If the point looked at be north or south, and near the horizon, the angle is small in the early morning, reaches a maximum about 9 or 10 a.m., becomes nought at noon, reaches another maximum at 2 or 3 p.m., and becomes

nought again at sunset. Towards the east or west no exact coincidence of the planes is observed, but there is a minimum about noon. In the morning and evening the angle between the sun's plane and the plane of polarization is tolerably large, as much as 6°; but near the time of coincidence there are perturbations which have prevented M. Becquerel following the movements of the plane of polarization near sunrise and sunset.

All the phenomena observed lead to the conclusion that the plane of polarization is twisted in the positive direction as seen by a person with his head towards the north and his feet towards the south, and that in the region perpendicular to the dipping needle the plane of polarization suffers practically no twist. The author is, therefore, led to the conclusion that this rotation of the plane is due to terrestrial magnetism. Certain investigations, not yet concluded, have enabled him to calculate *a priori* the possible amount of rotation producible by the earth's magnetism when the thickness of air through which the light comes is known.

This thickness has of course not been accurately determined; but by making certain hypotheses he has arrived at the result that the probable theoretical amount of rotation of the plane of polarization due to terrestrial magnetism is of the same order as the observed value. At the same time, the theoretical value is too small to enable him to conclude that terrestrial magnetism is the sole cause of the observed rotation.

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## W. E. AYRTON AND JOHN PERRY—THE CONTACT THEORY OF VOLTAIC ACTION, PART III.

(*Philosophical Transactions, Royal Society, Part I.*, 1880.)

Up to 1876 the whole theory of voltaic action was in a vague and incomplete state. Direct experiments on the difference of potentials of solids in contact with liquids had indeed been made, but all with indecisive results, owing to the imperfections of the apparatus and methods employed by the earlier experimenters, and the unjustifiable assumptions made. For instance, it was thought permissible to touch a liquid under test with the fingers, and it was assumed that blotting paper soaked in a liquid acted inductively as the liquid would do. Besides, great vagueness existed as to whether the contact difference of potentials (hereafter C. D. of P.) of two substances, A and B (for brevity $A\ \bar{B}$) was variable, as assumed by some, or a constant depending on A and B and the temperature, as Gerland considered he had proved, when one or both substances was solid. But the agreement of the E. M. F. of his cells with the sum of the C. D. of P. at the surfaces of separation is so striking as to cast doubt upon his conclusions, and as he did not experiment with two liquids in contact, he could not draw legitimate conclusions in the latter case. Kohlrausch alone had made direct experiments on the C. D. of P. of two liquids in contact, but as he employed surfaces of moist blotting paper, his results are not convincing.

To clear up the obscurity of the subject, the authors in 1875 designed an

apparatus and carried out experiments, whose results for 1876 will be found in Papers I. and II. (*Proc. Roy. Soc.*, No. 186, 1878); they measured directly the C. D. of P. in volts. at each separate contact of dissimilar substances, and proved the summation law that each surface of separation produced its effects independently of the rest. Thus, if $\overline{A\,B}$, $\overline{B\,C}$, &c., were separately measured (A, B, &c., being either solid or liquid), and if any number A, B, . . . K were joined together, the E. M. F. $\overline{A\,K}$ of the combination was found to be $\overline{A\,K} = \overline{A\,B} + \overline{B\,C} + \ldots + \overline{J\,K}$. After the preliminary notice of these results (British Association, 1876) Professor Clifton published a paper on the subject, and his results, as far as they go, are in general accord with the authors', although his apparatus, in spite of its exact workmanship, is objected to for the same reason as Gerland's, viz., the impossibility of measuring with it the C. D. of P. of two liquids in contact. The experiments, whose results are in the present paper, were made in 1877-78 with a new apparatus possessing important modifications suggested by experience. The principle of the method (which was inductive, since a conductive method would introduce unknown C. D. of P.) was as follows:—Two insulated gilt brass plates, 3 and 4, are connected one with each terminal of a quadrant electrometer, and underneath the plates are placed two substances in contact, 1 and 2, whose C. D. of P. is to be found. Their potentials are A and A + $a$. Two operations are now performed: first, 1 being under 3 and 2 under 4, the induction plates, 3 and 4, are connected together and then insulated, leaving them and the pairs of quadrants in connection with them at one potential B. Next, 1 and 2 are reversed in position, so that now 1 is under 4 and 2 under 3, and the potentials of 3 and 4 now become D + $d$ and D, and the needle is deflected. It is shown theoretically that $d$ will be proportional to $a$, or the deflection of the electrometer proportional to the C. D. of P. of the substances, provided the induction arrangement is symmetrical in both positions, or if the potentials A, B, and D are zero. Perfect symmetry being unattainable, an earth connection made A, B, and D zero.

The apparatus, which was very solidly constructed, may be thus generally described. A table carries levelling screws for supporting the experimental substances, say a metal plate and a liquid in a porcelain dish, and the induction plates are supported above them by vertical glass rods contained in an artificially dried space, the upper ends of the rods are fastened to a horizontal beam which has a vertical motion, obtained by an arrangement similar to that of a parallel ruler, thus enabling the induction plates to be raised or lowered together, so that the table which is centred on a pin turning in a socket and runs with wheels upon a horizontal railway may, when it is rotated in order to reverse the positions of the experimental substances, carry the vessel containing the liquid clear of the induction plates. There are besides various details for ensuring accurate performance, and the whole apparatus is enclosed in a zinc case connected with earth to avoid external induction, and is not opened during a complete experiment. After the insulation of the apparatus has been proved perfect, and the adjustments have been properly made, so that the induction plates are level and horizontal, parallel to and equidistant from the experi-

mental substances, the zinc case is closed, the induction plates connected together, and the electrometer reading taken. The induction plates are thus insulated and raised, the table rotated to reverse the positions of the substances, the induction plates lowered to exactly their former positions, and the reading taken again. After some ten readings in this way an equal number are taken commencing with the substances in reversed positions to correct for defects of parallelism, and the mean of the two sets is taken as the result of the experiment. The standard E. M. F. was a zinc-copper C. D. of P. amounting to ·75 volt, it having been found more constant than the Latimer Clark cell formerly used.

The results already obtained are (1), the C. D. of P. of metals and liquids at the same temperature; (2), ditto, when the substances in contact are at different temperatures; (3), the C. D. of P. of carbon and platinum with water, and with weak and strong sulphuric acid; but only those under head 1 are given in this paper. They are calculated first in chronological order, and next the mean C. D. of P. in volts. of solids with liquids and liquids with liquids in one table and of solids with solids in another. Very discordant results were obtained with mercury owing to small differences of temperature producing much variation. The C. D. of P. of the constituents in a Latimer Clark's mercurous sulphate cell also gave much trouble, on account of the thin layer of water which floated above the paste, but after many experiments conducted with great care $\overline{\text{Pt, Hg}}$ was found to be ·156 volts., $\overline{\text{Hg, mixture}}$, $\overline{\text{Amalg. Zinc}}$ ·200 and $\overline{\text{Amalg. Zn, Pt}}$ 1·125 volts. The sum of these is 1·481 volts., which should equal the E. M. F of the cell, about 1·46 volts.

As a test of accuracy, the sum $\overline{\text{A B}} + \overline{\text{B C}} + \overline{\text{C A}}$ for three metals should vanish. Some of the results are

$$\overline{\text{Cu, Pb}} + \overline{\text{Pb, Pt}} + \overline{\text{Pt, Cu}} = -\ 742 + 771 - {\cdot}228 = -\ {\cdot}009$$
$$\overline{\text{Cu, Zn}} + \overline{\text{Zn, Pb}} + \overline{\text{Pb, Cu}} = -\ {\cdot}850 + {\cdot}210 + {\cdot}542 = {\cdot}002$$
$$\overline{\text{Fe, Brass}} + \overline{\text{Brass, Cu}} + \overline{\text{Cu, Fe}} = {\cdot}064 + {\cdot}087 - {\cdot}146 = {\cdot}005$$
$$\overline{\text{Zn, Pt}} + \overline{\text{Pt, Cu}} + \overline{\text{Cu, Zn}} = {\cdot}981 - {\cdot}238 - {\cdot}750 = -\ {\cdot}007$$
$$\overline{\text{Zn, Pt}} + \overline{\text{Pt, Pb}} + \overline{\text{Pb, Zn}} = {\cdot}981 - {\cdot}771 - .210 = {\cdot}000$$
$$\overline{\text{Fe, Cu}} + \overline{\text{Cu, Brass}} + \overline{\text{Brass, Fe}} = {\cdot}146 - {\cdot}087 - {\cdot}064 = -\ {\cdot}005$$

Considering that the temperatures were not always the same, the smallness of the sums proves considerable accuracy in the experiments.

In all the experiments two air contacts enter; thus, where it is desired to measure $\overline{\text{A B}}$, what is measured is $\overline{\text{Air, A}} + \overline{\text{A B}} + \overline{\text{B, Air}}$, and similarly for B and C, what is measured being $\overline{\text{Air, B}} + \overline{\text{B C}} + \overline{\text{C, Air}}$ so that what is proved is that

$$\overline{\text{Air, A}} + \overline{\text{A B}} + \overline{\text{B, Air}} + \overline{\text{Air, B}} + \overline{\text{B C}} + \overline{\text{C, Air}} = \overline{\text{Air, A}} + \overline{\text{A B}} + \overline{\text{B C}} + \overline{\text{C, Air}}$$

which proves the summation law, whatever the C. D. of P. of a substance with air may be, as long as does not change, but it gives no indication of the actual value of the C. D. of P. at an air-contact.

When the C. D. of P of A and B is measured in a gas G, the result obtained

is $\overline{G\,A} + \overline{A\,B} + \overline{B\,G}$, and the difference between this and the air measurement is $\overline{\mathrm{Air},\,A} + \overline{B,\,\mathrm{Air}} - \overline{G\,A} - \overline{B\,G}$; so that it can be ascertained if the C. D. of P of a substance with a gas differs much with different gases. Mr. Brown of Belfast has already made *qualitative* experiments in this direction; and in a Crookes' vacuum the real value of $\overline{A\,B}$ might be reached, the same as the E. M. F. of the Peltier effect, so the authors intend to extend their quantitative experiments to other gases than air, and to a Crookes' vacuum, as well as to measure the Peltier effects.

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**E. MACH and S. DOUBRAVA.**—OBSERVATIONS AS TO THE DIFFERENCE IN THE ACTIONS OF POSITIVE AND NEGATIVE ELECTRICITY.

(*Annalen der Physik und Chemie, B.* ix. *H.* i., *No.* 1, 1880, *pp.* 61-76.)

The authors commence by referring to the experiments of Mach, Doubrava, Lullin, Lichtenberg, saying that the phenomena observed would appear to be enigmas, not subject to the present mathematical theory of electricity. They draw attention to the fact that, if any one set of phenomena be taken by themselves it is easy to set up a plausible theory, but that the only way to arrive at the true explanation is to seek for one common source of all the phenomena. Reitlinger first pointed out a parallelism between Lichtenberg's figures and Faraday's luminous brushes, and he sought for an explanation in the motion of the air particles from the positive electrode. Plücker opposes this view, and looking at the experiments of Bezold says, that too little attention had been paid by Reitlinger to the nature of the dielectric. Thus again, in Lullin's experiment which consisted in piercing a card by a discharge of electricity between two points on opposite sides of the card, but not quite opposed to one another, the hole is always found opposite the negative point. This was explained by Reitlinger to be due to the greater length of the positive brush; but the experiment succeeds equally well in carbonic di-oxide where the brushes are short. Again Waltenhofen has shown that if we replace the card by some other substance, such a paper, the hole is made opposite the positive point. He came to the conclusion that the hole would be opposite the negative point in the case of all substances, which when rubbed with damp air become negative, and that it would be opposite the positive point, when the substance pierced was such that it became positive when rubbed with damp air. But this theory is incorrect, for the phenomena cannot be altered by electrifying the substance, or when we produce little explosions to assist the rush of air over the surface. The authors think that not only the surface but also the internal constitution of the card must be taken into account. They then proceed to describe numerous variations of Lullin's experiments, using a great number of points by placing strips and sharp pointed rhombs of tinfoil upon the card, and by cutting little slits in the card. Such slits are only used by the spark if they come from negative poles. It seems then that paper

having negative potential has a greater electrical firmness then when it is positive. They thought it worth while to test this assertion which would explain many phenomena, and has a relation to the fundamental fact that of two bodies rubbed one becomes positive and the other negative. Doubling the card so that greater thicknesses are opposed to the negative point; finding if there is increased heat when a card is opposed to the positive point, or finding if in such a case there is greater potential difference before discharge; using double pointed discharging rods with card over one negative point and card over one positive point, and finding the negative card pierced, and finding this is not the case if little balls are used instead of spheres; after all these experiments the authors say that although the above assertion may be kept up in a forced way, we have not gone much beyond Waltenhofen's assertion that Lullin's results depend on the material. The authors now proceed with centric symmetric arrangements. If a small sphere $a$ is one electrode, and a larger hollow concentric sphere A is the other, then a discharge more easily takes place between — A and + $a$, and indeed, between + A and — $a$ it never becomes a spark but only a brush discharge. Giving well known mathematical expressions for electromotive force between the spherical surfaces, the authors show that the rate of change of potential is no criterion of discharge. Both Righi and the authors made many analogous experiments, Righi's explanations of which are not satisfactory to the authors. Righi found a centric symmetric electrode of arrangement of the above kind of two metals A and B, such that whether A or B forms the material of the outer electrode, the discharge takes place more easily between + A and — B than between — A and + B.

The authors now proceed to consider length of spark discharge. Using a spark micrometer they found that it required a greater difference of potentials to produce a spark through a given distance, when the Leyden jar was charged positively, than when it was charged negatively in the relation of 5 to 4. [The disposition of the apparatus is not quite clear. Compare experiments by Drs. De la Rue and Müller, Phil. Trans., Vol. 169.] If we discharge a plane condenser between two similar small ball electrodes, we find same length of spark when the coatings are at the potentials $+\frac{V}{2}$ and $-\frac{V}{2}$ or $-\frac{V}{2}$ and $+\frac{V}{2}$, but if we enclose the condenser in a case which has the potential $+\frac{V}{2}$ or $-\frac{V}{2}$, so as to make the coatings O and + V or O and — V, we get unequal lengths of sparks although this case seems to be only immaterially different from the other The authors here point out the difference in character between the equipotential surfaces in the case when the discharging balls are $+\frac{V}{2}$ and $-\frac{V}{2}$, and when they are + V and O, and show that this ought not to be of any consequence. They then surrounded their discharging balls only by a metallic case, which could be connected with one of them or to the earth, and found a difference in the length of spark

depending on which of the two electrodes was positive. They describe a modification of this experiment. Again they enclosed the apparatus and themselves in a conducting case. They also took greater care in making experiments on length of spark with their micrometer arrangement. They revert to their consideration of fall of potential in the neighbourhood of an electrode, saying that there may be a constant in the expression for distribution of potential, which does not change sign when we change the signs of the electricities, but they say that this cannot explain the phenomena observed.

Doubrava made some experiments alone. A water pipe 6·8 inch in length, 3 inch in diameter, connecting the electrodes of a Holtz machine, showed at the middle a negative potential, and the zero was always nearer the positive than the negative electrode, and when the electrodes changed in sign the zero moved about one metre. A metal plate placed midway between two pointed electrodes had no charge if immersed in oil of turpentine, but was found to be positive if immersed in olive oil. When he surrounded the movable parts of his electroscope with oil of turpentine, it measured charges as usual, but if he used olive oil he found a difference of readings in the proportion of 3 : 4 according as his movable parts were positive or negative. The authors say that in all these experiments the influence of the material is evident, but the explanation is as yet unknown.

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ıe Eighty-sixth Ordinary General Meeting of the Society was held on Wednesday evening, March 24th, 1880, at the Institution of Civil Engineers, 25, Great George Street—Mr. W. H. PREECE, President, in the Chair.

The Theatre of the Institution was lit up by two Electric ımps on Mr. Crompton's principle, and for a short time by one . Mr. Heinrich's principle.

After the ordinary business had been transacted, the Discus- ›n on "Some Recent Improvements in the Electric Light" was ntinued from last meeting.

Mr. CROMPTON remarked that the two lamps then burning ere constructed on the principle he explained last meeting. Each mp was supplied with a current from an A size continuous ırrent Gramme machine.

Mr. BERLY, who was unavoidably absent, sent the following:—

## OTES ON THE JABLOCHKOFF SYSTEM OF ELECTRIC LIGHTING.

In October, 1877, the Halle Marengo of the Magasins du ouvre was lighted by means of six Jablochkoff electric lights, .e first time that the Jablochkoff system of electric lighting was ıblicly exhibited.

Since that date, the Administration of the Magasins and :otel du Louvre have gradually extended the lighting of their

establishment, and there are now not less than 120 lamps distributed all over the place, 84 of which are burning at a time.

The Louvre establishment has had a special 100 horse-power steam engine fitted up in the basement, for working their electric lights.

The light has, from the beginning, worked uninterruptedly every night—that is to say, for a period of nearly two years and a half.

In February, 1878, the Avenue de l'Opéra, Place de l'Opéra, and Place du Théâtre Français were lighted, for the first time, by means of 64 Jablochkoff electric lights.

Since that time, that is to say for more than two years, the lights have been uninterruptedly kept burning: and the 69 lights which are now burning—32 in the Avenue itself, 16 on the Place de l'Opéra, 15 on the Place du Théâtre Français, and six on the frontage of the Opéra itself will continue to shine for at least one year, for the Paris Municipality has just renewed the contract with the Société Générale for one year ending in March, 1881.

A little later, the Hippodrome in Paris was electrically lighted by means of 20 Serrin regulators (each of which required a special Gramme machine to drive it, necessitating therefore 20 machines) and 60 Jablochkoff lights worked by three batteries of 20-light Gramme machines. These lights have been kept, since that time, uninterruptedly burning every day, Sundays included—that is to say, for a period of two years; and the Administration of that establishment has just decided to take down the 20 Serrin regulators, and has contracted with the Société Générale to replace them by 68 Jablochkoff lights, which is being done; so that that immense place will in future be lighted by means of 128 Jablochkoff lights.

In October, 1878, the Metropolitan Board of Works contracted with the Société Générale d'Electricité for the lighting, by means of 20 lights, of the portion of the Thames Embankment comprised between the Bridges of Westminster and Waterloo. The inauguration of that lighting took place on the evening of the 13th December, 1878.

The 10th May, 1879, the lighting of the Embankment was extended from Waterloo to Blackfriars Bridge, 20 lights being added to the 20 already burning.

A further extension took place the 10th October, 1879, ten more lights being placed on Waterloo Bridge, and bringing the total of the lights to 50.

Since these various dates—that is to say, for fifteen months for the first series of 20 lights; ten and a half months for the second series of 20; and five and a half months for the last series of 10—the whole of the lights have been uninterruptedly kept burning every night, Sundays included, for six hours, which is the contract time for keeping the lights burning.

These facts are worthy of consideration, when it is borne in mind that the plant had originally been laid down for 20 lights only; that the extension of the lighting not having been contemplated at the time of the plant being laid down, no provision at all was made in case such an extension should take place. This necessitated the manipulating and altering of the plant each time, as the best that could be done under the circumstances. Also that another 10 lights (making 60 altogether) were subsequently added to the 50 already burning.

On the 20th December, 1878, Messrs. Shoolbred & Co., the well-known drapers of Tottenham Court Road, London, purchased a plant for lighting a portion of their premises by means of six Jablochkoff electric lights. One week after, they ordered another 6-light plant; and a 20-light machine having been delivered to them, instead of two 6-light ones, which would have cost as much, they extended their lights to 15, then to 20, as soon as a special steam engine which they had procured to work the light was fixed and ready for work.

They have now 25 lamps, 20 only burning at a time, and are so pleased with the light that they would have further extended it last season, had they not been afraid of being interfered with in their business by the carrying out of the necessary alterations; they are, however, contemplating its extension for the coming season.

Their lights have been burning uninterruptedly for these last

fifteen months, the only stoppage occurring being during the longest days of midsummer; and it is worthy of notice that during the fogs of the past winter, for three weeks together, the lights were kept burning from ten a.m. till seven p.m. without a single interruption.

At the same date the Aston Lower Park Company of Birmingham purchased a plant for lighting their admirable pleasure grounds, aquarium, skating rink, theatre, &c., with 40 lights of the Jablochkoff system. Since that date, that is to say, for a period of 15 months, the whole of the lights have been uninterruptedly kept burning.

The same may be said of Messrs. Wells & Co., marble merchants, of Shoreditch, who have been using 6 Jablochkoff lights since October, 1878: of Messrs. Crocker & Sons, of Friday Street, City, who have had the whole of their splendid warehouse fitted up with 20 Jablochkoff lights working since nearly one year ago: of Mr. Nicol, tailor, in Regent Street, who has been using four of these lights in the windows of his large shop since the beginning of November last, and of Messrs. Samuel Brothers, tailors, in Ludgate Hill, in whose shop 13 have been burning since the middle of December last.

The annual picture exhibition known as the "Salon," in Paris, and which is held at the Palais de l'Industrie during the month of June, is under the control of the Ministry of Beaux-Arts, who last year only signed the authorisation for lighting the Palais de l'Industrie on the 1st June, the very day of the inauguration

The work of erecting steam power, electric machinery, fixing the electric lamps, and the necessary conducting wires, was carried on during seven consecutive days and nights without a stoppage; and on the 8th June, the necessary steam and engine power to drive 16 batteries of 16 lights each—the said number of batteries of dynamo-electric machines having been got ready, some six or seven miles of conducting wires fixed, and all the lamps placed and connected, 256 Jablochkoff electric lights illuminated nightly the said Exhibition from that date up to the end of the month—not less than 117,509 persons visiting it at night and paying each one franc admission.

The authorisation for this year having just been obtained, the Société Générale is preparing for lighting the coming "Salon" with 400 Jablochkoff lights.

Many places have been lighted in Paris and in France for spaces of times as long as those above mentioned, and especially the Theatre du Châtelet, in Paris, where 16 lights have been burning outside, inside in the auditorium and on the stage, and this with a great success for two years. There are actually—

465 lights of the Jablochkoff system burning in Paris.

428 lights of the Jablochkoff system burning in the French provinces.

823 lights of the Jablochkoff system burning in foreign countries.

Making a total of 1,716 lights, 198 of which are actually burning in this country.

The small proportion of lights burning in Great Britain, compared with other countries, is owing to the fact that the French Society is more of a demonstration Society in this country than of a commercial concern, as it has always been intended to work this system with a large English Company, which has only been recently constituted.

The Metropolitan Board of Works has renewed for one year the contract with the Société Générale for lighting the Thames Embankment and Waterloo Bridge; the new contract expiring on the 10th April, 1881.

The price paid by the Metropolitan Board of Works was originally 6d. per hour per light per lamp. This was reduced to 5d. per hour per light on the Board extending the number of lights to 40; then to 3d. on the lights being extended to 50, and the contract renewed for six months. It is now to be carried out at 2½d. per light per hour, the contract having been extended to one year, and the Société Générale contemplating extending the number of lights to certain private concerns from their centre at Charing Cross.

The Jablochkoff system of electric lighting is now used in almost every description of establishments, places, or trades, &c.,

in shops (largely), in streets, in squares, bridges, in theatres, circuses, engineering works, blasting furnaces, dyeing works, weaving and spinning mills, laundries, foundries, hotels, private residences, paper manufactories, docks, basins, on board steam vessels, in building yards, pleasure gardens, optical works, public establishments, museums, exhibitions, railway stations, &c., &c. Even crowned heads have taken advantage of it; for their Royal Highnesses King Thebaw of Burmah has had 60 fitted up in his palace at Rangoon; Shah Nasser ed Deen of Persia, four in his palace at Teheran; Prince Agakhan, 6 in his palace at Bombay; King Dom Luis I. of Portugal, six in his royal palace of Cascaes; and the unfortunate ex-queen, Isabella of Spain, six in the conservatory of her beautiful residence in Paris.

The first Jablochkoff candles made were sold at 8d. each. The price was soon reduced to 7½d., then to 6d.; they lasted, at that time, 1½ hour. Successively the price was reduced to 5d. and 4d., at which it now is, and at the same time the duration was increased from one and a half hours to two hours; so that it can be said that the cost for the candle only, which was, during the winter 1878-9, 5d. per hour per lamp (7d. for one and a half hours), has been reduced during the following year from that price to 2d. per hour (4d. for two hours), or 60 per cent. reduction in price.

The cheapening has been brought about by the candles being successively improved, and from the quantities manufactured being constantly increasing.

The increase in their duration has been obtained by simply covering the carbon with a film of copper by means of the electroplating process, which so little increases the cost that they are sold at the same price as the non-coppered ones.

The two imperfections with which Jablochkoff system is reproached are, viz., the fact that the whole circuit goes out if one single candle in the said circuit does, and the inconvenience of switching a new candle into circuit every two hours, have not been lost sight of by the Société Générale and its staff.

Although the first of these two inconveniences is of no great importance, since in practice the single lights do not go out, still an efficient remedy has been found in a candle which relights

itself; which I illustrated at the British Museum in March, 1879, when carrying out the experimental lighting of the Reading Room of that establishment, and to which Mr. Keates, the Chemist to the Board of Works, referred in the evidence given by him on 16th May, 1879 (I think No. 1,437), before the Select Committee on Lighting by Electricity.

As to the second one, numerous arrangements have been devised to do away with it, and some of them are nearly as perfect as could be desired. Something as simple as the candle itself being required, to be effective and practical, the more or less ingenious devices, some of which are extremely ingenious, have not yet been put into regular use; but there is now a plan under trial at the works of the Société Générale which, it is believed, will definitely and satisfactorily solve the question.

Had I been allowed to bring inside this room a pair of conducting wires from our centre at Charing Cross, I would have, with other illustrations, shown an automatic switch which is so simple and effective in its action that a fresh candle is without hesitation brought into play, if the burning candle goes out or is burnt down. I cannot yet describe it, but will say that it is not likely to be adopted in its present form. It is, strange to say, simpler than the lamp itself without the switch; and the lamp with the said switch would undoubtedly cost less than without it. However, it is deficient in one respect, and consequently not perfect.

The great impetus given to the question of electric lighting, and to which the Société Générale on the Continent and the Société Générale and the Board of Works in this country have contributed a very large share, has, besides the direct result of promoting important improvements in the various more or less successful systems of electric lighting now in use, had also the indirect result of promoting researches and improvements in the various apparatus connected with the solution of this important question.

The beautiful alternating dynamo-electric machine of Mr. Gramme was devised by him especially to suit the Jablochkoff candle, which requires an alternate current to work it.

The more recent progress made by Mr. Gramme in combining into one his two admirable continuous and alternate current machines, and producing his self-exciting machine, is also a great improvement, as it cheapens the cost of the machines, simplifies the arrangement of foundation and driving gear, occupies less room, requires less power to drive, and consumes less oil, &c.

I am justified in saying that before long I expect to be able to announce that the candle will last three hours instead of two; and as they will be sold at the same price, an important and real improvement will be produced.

Great and constant improvements have also been made in the details of the machinery, plant, &c., connected with the Jablochkoff system of electric lighting. These improvements are constantly being made, the majority of them too unimportant to be made the subject of a patent; but, taken as a whole, they certainly represent a respectable amount of ingenuity, and have contributed in a large proportion to make the question of electric lighting what it now is.

I should not omit to mention that the gas engine has been a powerful auxiliary to the question of electric lighting, and its use is spreading at a very great pace.

At the time when the question of lighting a portion of the Thames Embankment was first mooted, a great controversy, degenerating in a paper war, took place respecting what could and what could not be done.

According to some, the idea of lighting a frontage of about 1,200 yards from one centre was preposterous, as it was a well-known fact that a light could not be maintained with an alternating current at a distance of more than a couple of hundred yards. According to some others (I must confess that they are gas engineers), who went to the trouble and expense of publishing a pamphlet to prove it, for certain reasons, not less than 1,700 stations, requiring 1,700 engine drivers and 1,700 two horse-power gas engines, would be required to light only the streets of London, and I naturally shrink from telling at what rightful expenses this result was to be obtained. Others predicted that all the current would be lost if any attempt was

made at dividing it, and there would consequently be no light, &c., &c.

Since that time, however, and notwithstanding these by no means mild and friendly criticisms, the following facts have been proved :—

That 80 Jablochkoff electric lights could be maintained with a 20 horse-power portable engine, an experiment which I carried out Monday, the 22nd inst.

That a frontage of more than two and a half miles of electric lights (the distance from the first lamp at Blackfriars Bridge to the last lamp at Victoria Station) can be maintained from one centre at Charing Cross.

And to persons who had their doubts about the possibility of keeping alight the last lamp at Westminster, which is about 700 yards from the centre, it has been proved that a circuit of five lights could be maintained on a length of wire equal to seven miles (an experiment which I carried out on the 2nd and 4th of March by connecting together the two circuits used for lighting Victoria Station from Charing Cross), or a radius of three and a half miles. This experiment, as well as those which I have made from time to time, increasing the distance gradually every time, have all been carried out with the same size of conducting cable, viz., a strand of seven wires of No. 18 B.W.G.

A circle of three and a half miles radius, traced on the map of London from Charing Cross as a centre, will give an idea of the immense area which could actually be lighted, if necessary, from one centre.

Further progresses are in store ; the apparatus connected with electric lighting is daily improving and becoming cheaper as its use is progressively growing in importance ; new discoveries and inventions are constantly being made and published ; and supposing that we take it for granted that the question of electric lighting will not progress more rapidly than the other great discoveries and inventions of this century, one may fairly expect that it will, within a very reasonable time, as surely become of general and everyday use, as it has (as I think I have been able to establish in these short notes) now passed the period of experimentation and entered on a period of practical and beneficial use.

Mr. J. MACKENZIE, in describing the principle of his lamp—a specimen being on the table—said: The carbons are placed point to point one over the other. The upper carbon is held in a tube by a cam which is fixed on a lever. At the end of the lever the keeper of an electro-magnet is fixed by a rod. This keeper is attracted while the current is flowing, and the arc is maintained at its proper length, but if the carbons are consumed so that no arc is produced, or the current becomes too weak, then the circuit is broken, the keeper is released, which allows the upper carbon to fall, producing contact. Then the current again flows and the upper carbon is held in position by the attraction of the keeper to the electro-magnet which moves the lever and cam holding the carbon. This arrangement produces a jump in the light about every 15 or 20 minutes, according to the consumption of the carbon, but the jump is very quick and hardly noticeable.

The lower carbon is pushed up by a spiral spring, to which is attached a ratchet chain and escapement. The magnet which moves the upper carbon holder also moves this ratchet, and the lower carbon is slightly lifted upwards as the upper carbon comes down.

Such an arrangement permits of long carbons being used, and the lamp now exhibited has been alight for half an hour this evening, giving about 1,600 candle-power. The lamp has also on several occasions been burning for an hour and a half in a lantern over our gateway.

CHARLES F. HEINRICHS:—I have to-night a current supplied to my lamp more suitable to the carbons than the current I could make use of in the last meeting, therefore you see the lamp successfully burning. The principle of my lamp differs from that of all lamps previously used, and from those exhibited and shown here to-night. In 1813 Sir Humphrey Davy used in his electric lamp two straight carbon pencils vertically placed above each other, and since that time two or more straight carbon pencils vertically placed above each other have been used with lamp mechanisms, differing more or less from one another. Now, in my lamp the carbon pencils are made into a circular shape, and the mechanism which separates and feeds the carbons are quite novel and effective. The circular

form of the carbon pencils has many advantages, while the employment of straight carbon pencils has many disadvantages. The lamp I have here on the table is an improved Serrin lamp, in which straight carbons are employed, and where you can see the disadvantages of the class of lamps where straight carbons are employed. The rack which guides the carbons must be somewhat longer than the carbons, which will not allow a great length of carbon to be employed. The rack is in gearing with the regulating mechanism into which it carries the dust of the carbon, and thus causes irregularities and stoppages. Such a lamp cannot be made to burn safely for a longer period than six hours, and then its size will be twice or three times as large as a lamp of my construction, having a burning time of eighteen hours. In the lamp which burns above me two semicircular carbon pencils are employed, and make nearly a full ring of 12 inch diameter, which gives a length of 36 inches carbon, and which will burn for a period of 12 hours. In a new modification which I hope to show you in a short time I can employ nearly two rings of 12 inch diameter in the same space as the lamp occupies you see burning, which thus will increase the burning time up to 20 hours. I can easily construct lamps that will burn up to 30 hours or more. The arm which guides these lengths of carbon will only be 6 inches long; no dust will be carried into the lamp mechanism in my lamp, and thus the greatest source of stoppages is overcome.

The regularity of the consumption of the carbons depends upon its density. The straight carbon pencils are made in the following manner, viz., the prepared carbon mass is pressed through the discharge passage of a cylinder by a piston, and by this process the density of the pencils cannot be uniform, because the first end forced out cannot have the same density as the latter end when there is less mass under the pressure of the piston. Now my carbons, which are made firstly in a ring, I afterwards submit in circular dies to an equal pressure all round, and thus the density of the carbon is equal at all parts. Vertically placed carbons consume three inches per hour, while my circular carbons burn at the rate of two and a half inches per hour; this difference is due to the position of the carbons in the arc. In vertically placed

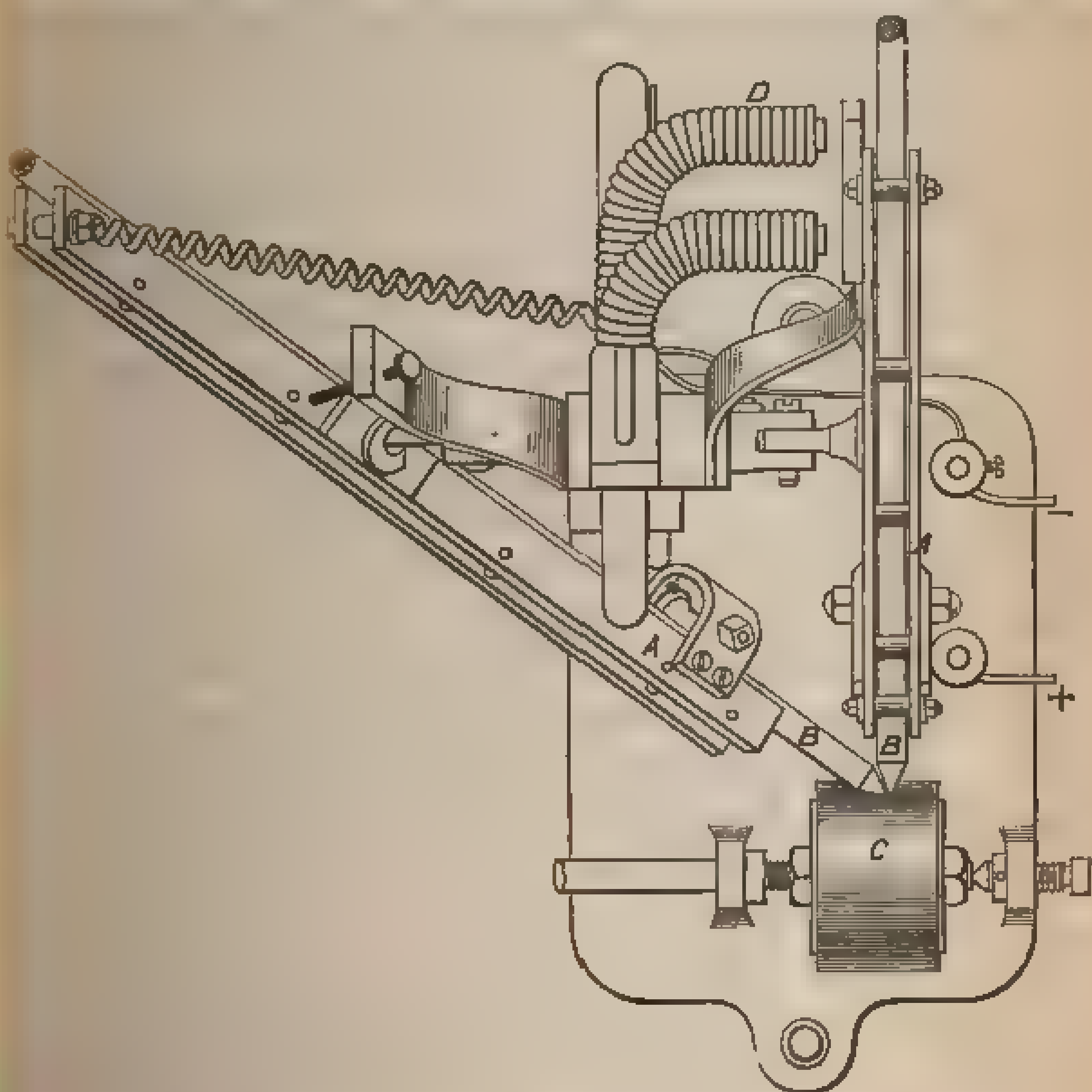
carbons the arc wastes the upper carbon, while the feed of the circular carbon is horizontal to the arc, and therefore no waste of the carbons. I hope shortly to improve the carbons and to bring out more lamps, using the circular carbons.

Mr. KILLINGWORTH HEDGES, while thanking Mr. Alexander Siemens for his paper, would like to ask him if the comparative figures of cost in the case of the Blackpool lighting include only the supposed value of a gas illumination of equal power to the electric light, or the actual cost of the gas disused? It seemed to him when at Blackpool, shortly after the lights had been started, that if gas burnt in improved burners had been used, a better effect than that from the electric lamps would have been produced at a less cost. The system adopted of fixing a strong electric light on the top of a high pole was certainly more economical than that of having a number of lights, as on the Thames Embankment. With these light centres, as at Blackpool, however, the dark spaces between the lamps were very noticeable, and immediately under each light the effect was too powerful. Mr. Hedges had noticed this effect more or less in all instances of street lighting by electricity. Standing at the junction of the Avenue de l'Opéra with the Rue 4me Septembre, in Paris, the difference of the lighting between the former street by electricity and the latter by gas was most remarkable.

Mr. A. Siemens in his paper did not mention any improvements in the electric light with reference to the colour and frequent hissing. Both these points are most important, and have been mentioned to him frequently as strong reasons against the advancement of the light with the general public. The hissing is very objectionable, and the blue colour often produced with strong lights causes the ghastly effect complained of by the ladies. In the lamps in use here, Mr. Crompton appeared to have got over these difficulties to a large extent. Mr. Hedges thought that the best way to use the light was by reflection—that is, throwing the rays upwards on to a luminous ceiling, and thus dispersing them. This mode of lighting had been successfully applied in the Picton Gallery, Liverpool, where the whole of the light was thrown upwards.

He found that by introducing certain metallic substances into the composition of the carbons, or by coating them afterwards, the effect of the violet chemical rays could be almost entirely annulled. The pure white colour of the electric light is thought to be cold by persons who have been accustomed to gas for a long time, and are not ready to adapt themselves to a new light of a different colour. He thought that by slightly colouring the electric light, this objection would be gradually overcome, and after a time the perfected pure white light would be accepted.

Mr. Hedges spoke of experiments made with different materials in connection with a new simple form of electric lamp, which he




described. It consists of two tubes or troughs A A, inclined at such an angle that the carbons B placed therein fall by their own gravity, until they meet at the circular block C, which is composed of a refractory material. The troughs are insulated one from the

other, and one is arranged to move sideways when acted upon by the electro-magnet D. The other trough can also be moved slightly for adjustment. The electric current passes down the trough A, through the electrodes B, then round an electro-magnet which separates the carbons and produces the arc. Some difficulty at first was found in getting a material for C to stand the intense heat and disintegrating action. Magnesia and lime give the best results, and largely increase the light; but to avoid disintegration, it is necessary to give C a slow rotary movement. With the lime block there is an absence of the blue effect complained of, small particles of lime probably being consumed and changing the colour. An experimental lamp was shown, adapted to fix against a wall for factory lighting, three of which lamps could be worked from one continuous current machine of the ordinary type. The cost of this form of lamp was stated to be one-quarter of that of the Serrin type.

Mr. H. F. Joel: Before explaining the recent improvements in Mr. Werdermann's lamp, I am desired by Mr. Werdermann to express his regret at not being able to be present to personally explain the improvements.

Mr. A. Siemens stated that the only improvement in the Werdermann light was the substitution of a copper disc for the one of carbon. I hope to convince you that this is only one of the minor details of the improvements in Mr. Werdermann's system of electric lighting. The whole result of these improvements has been that whereas formerly with a quantity-current machine we could only get 320 candle-power per horse-power, we can now get 750 candle-power, or over 100 per cent.

The original Werdermann light is shown by the following sketch.

Fig. 1 shows a side elevation. The clamp or ring *b* carries the carbon disc *a*, which is fixed in it by the screw *s*, this forms the top electrode; the carbon pencil *e* is pressed upwards against the disc *a* by a weight (as is well known). The contact between the pencil or rod *e* and the copper nipples *g g* is made by means of the spring and screw *l l′* and the separate copper block *g′*

(forming part of the nipple). The tension put on this spring *l* regulated the upper pressure of the carbon rod and the contact nipples.

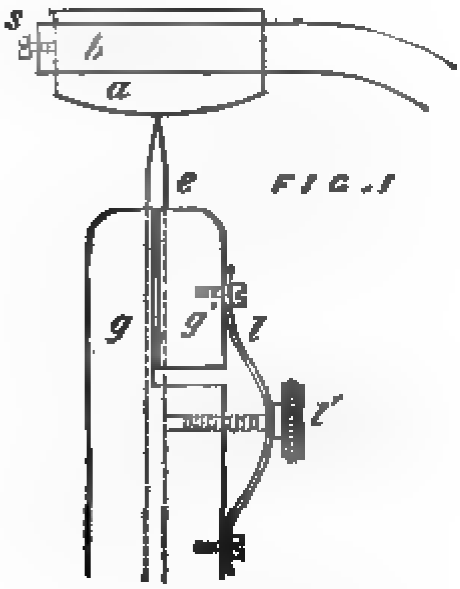


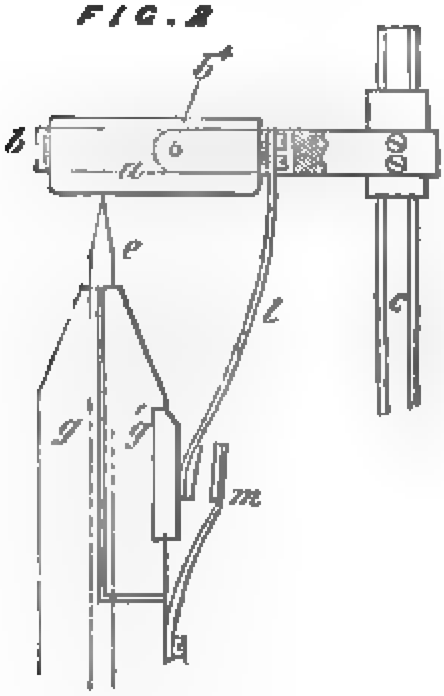

FIG. 1

SIDE ELEVATION

In practice it was found impossible to burn these lamps in series with an intensity current, because the spring would cause the carbons to stick, thus preventing several lamps being joined into one circuit.

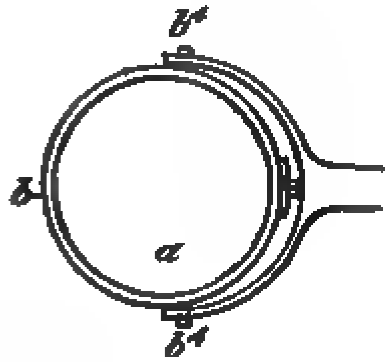
The improvement on this arrangement was as below :—

FIG. 2




SIDE ELEVATION

FIG. 3




PLAN OF TOP

Fig. 2 shows a sectional side elevation, and Fig. 3 a plan of the top. The clamp or ring *b*, which carries the copper disc *a*, is supported excentrically on pivots or spindles $b^4$, fitted in a hori-

zontal arm extending from the rod or bar *c*. The excentricity of the copper disc is such that its weight just preponderates on the side with which it is in contact with the carbon rod *e*. The spring *l* is fixed on the ring or clamp *b* of the disc *a*, and bears against a piece of insulating material on the movable half of the copper nipples *g'*. When the carbon pencil is nearly all burned, the spring *l* rests against the projecting piece *m*, and so short circuits the lamp.

This arrangement of automatic regulator allows the carbon pencil a free upward motion, and there is no risk of breakdown, and the lamps can by it be all joined in one circuit and worked by an ordinary dynamo-magnetic machine such as the small A Gramme, the current obtained from which machine being sufficient in quantity and electromotive force to keep ten good lights burning of about 200 candle-power each. And further, an automatic short circuiting contact (shown in the sketch) is fitted to the lamps. By this arrangement, when the carbon pencil in the lamp is burned out, a short circuit spring comes into play, and allows the current to pass on to other lamps without causing breakdown.

In Paris, lamps constructed on this principle, and of the accompanying form, are used, and experiments made by Mr. Werdermann with intensity machines of either the Gramme or Siemens type show that each lamp gives a light of 750 candles per horse-power, a result which was verified by one of the French Government officials.

One of the improvements I have made does away with the

trouble of making connections; the act of fixing the lamp in a clamp makes the double connection.

Another of my improvements is an arrangement for putting the carbons in a tube or sheath from the lower end, avoiding the necessity of removing the globe to supply new carbons.

Another improvement is in the arrangement of the resistance of the lamp, which I found by experiment with 14 mm. carbons, 18 inches long, to be when not burning 39 ohms, and when burning 16 ohms; this lowering of resistance having a most important influence upon the economy of this light.

The last improvement I would mention is the introduction of graphite nipples in the copper contacts. It was found that the copper contacts after a little time became clogged; this is not the case with graphite.

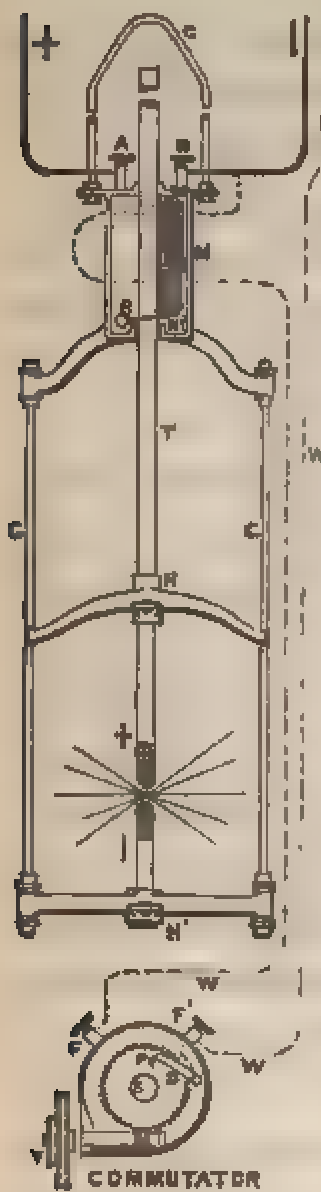
These are the recent improvements made in the Werdermann electric light system, and they have made his lamp much more perfect and capable of being easily attended to and understood.

Professor AYRTON: Will you kindly explain how you determined the resistance of the arc? It is rather a difficult thing to determine the resistance of an electromotor possessing an electromotive force which varies according to the current used.

Mr. JOEL: It was determined by the method of substitution. Ten lamps were taken, and by turning nine of them out it was found that the tenth remained constant; and so on taking any other of the lamps in circuit and turning the remainder out, the same constant was observed. That seemed the most practical way of testing it exactly.

Mr. S. E. PHILLIPS, Jun., described Mr. J. A. Brockie's lamp, and said he considered this lamp quite a new departure in electric lighting, since the regulation was not brought about by alterations in the strength of the main current, but was performed at definite prearranged periods, by means of a branch circuit and a simple commutator which interrupted the current at regular intervals. The positive or upper carbon is attached to a metal rod passing through a sudden-grip clutch, which may also be the keeper of the electro-magnet. When the current passes through the lamp,

the small portion going through the shunt circuit, formed by the electro-magnet and commutator with the necessary wire connections, acts on the clutch and strikes the arc. At any prearranged period, for instance every minute, the current passing through the electro-magnet is interrupted, the positive carbon-holder is released, and the two carbons fall together—to be again instantly separated, as the current is only interrupted for the shortest possible period. This action, which takes place with great certainty and regularity, does not produce any noticeable blink in the light, nor does it cause any sparking at the commutator of the dynamo-machine. The great advantage of this lamp is its extreme simplicity; also that they can be burnt in series. One of Siemens' small machines recently worked two of these lamps in series, producing a perfect arc in each.




Mr. CONRAD W. COOKE said that although the electric arc was discovered seventy years ago it had lain dormant to within a recent period. This was because no inducement, in the shape of public interest, which was always accompanied by remuneration in case a remedy was supplied, had been held out for inventors to apply their genius to adapting the light for useful purposes. When the public interested itself in the subject, the patent office was soon inundated with electric lighting specifications. Electric engineers were indebted to the Société Générale for bringing together the exhibition of 60 or 70 different kinds of lamps, and demonstrating the practicability of street lighting in Paris, at a time when universal representatives of all classes were congregated in that city. The improvements in the Jablochkoff system, made by Mr. Berly, were of the greatest importance, as they enabled a large radius to be illuminated by power developed at a given centre. For instance, the twenty horse-power engine at Charing Cross

supplied currents to Victoria Station and the whole of the Thames Embankment, a distance of three and a half miles, and such a radius represented an area of forty square miles. The Lontin system was one of the earliest introduced in this country, but had made little progress, except its employment on the Metropolitan Railway; and the Rapieff, too, though ushered in with influential patronage had not been used outside the *Times* office, where it had given great satisfaction, and where its use had been extended to a greater field than it was at first employed in. He was sorry that Mr. Siemens had not dealt with the subject more generally in his paper, and he also regretted that the Brush system had not been exhibited, as for this system he predicted a great future. The Werdermann light stood in an intermediate position between the arc and incandescent lights, as from the fineness of the carbon incandescence was produced, while at the same time it could not be possible for such a thin carbon to carry the whole of the current, and an arc was produced. On examination of a Werdermann lamp with a dark glass he had noticed a discharge going on round the carbon. This light was very steady and was much appreciated in Paris, and had recently been adopted by the Kensington Museum authorities. In Mr. Crompton's lamp the question of regulating the arc was practically solved, while in the Siemens' pendulum arrangement he considered too great an interval occurred between the feeds.

Mr. Ladd: As regards the "Brush" light, I would say the only reason that has retarded its progress has been the necessary delay in the formation of a company. The Board of Directors was only completed this day, consisting of well-established business gentlemen who will push the matter forward, and the Brush light will soon appear more publicly than it has hitherto done. An improvement has been made in the lamp, which now burns perfectly steady, as anyone will see on paying a visit to the Liverpool Street Station, where the improved lamps have been burning since Monday last.

By the Brush light system the carbons cannot separate beyond a certain limit, which is about 2 mm. We have Brush lamps that will burn 16 hours without attention. The sum total of light varies but very little as the number of lamps is increased

or diminished; if 16 lamps are in circuit, and 4 or 5 additional ones are switched into circuit, the carbons approach a trifle closer together, and the light of each lamp is a little diminished. But if 4 or 5 of the 16 are taken out—which can readily be done, without breaking the circuit—an instant separation of the carbons takes place, giving increased illuminating power to each lamp, all the lamps being equally affected.

There are 16 lamps of this system now in use at Liverpool Street Station, 16 at Messrs. Peak, Frean & Co.'s Biscuit Works, and 16 are burning at Messrs. Crossley's Works, Halifax; all giving complete satisfaction. Eight will be brought into operation directly at South Kensington Museum, and many orders are in hand.

We have not pressed the Brush system on the public by demonstration; having entire confidence in it ourselves, we preferred to let it assert its own claims to public approval.

As regards the candle-power, I measured one of 16 lights myself with a Bunsen photometer, and found that it was 971 candles; the measurement was taken on a level with the light. The carbons were adjusted in a line with each other, so as to give equal light all round. This is not the way in which the candle-power is always measured, and it would hardly be the just way, because in single-current machines the top carbon is very much the hottest, and there is a great deal more light, which (as every one knows who has had to do with single-current machines) is thrown down; and thus, at an angle of about 45°, those 16 lamps would each measure 2,000 candle-power, and I will vouch for that as being a fair estimate of the light. The same result has been arrived at by independent men. The whole 16 lamps would, therefore, give 32,000 candle-power. The machine is worked by an 8 horse-power engine, indicating 14 horse-power, made by Wallis & Stevens, which works steadily, and keeps a steady action on the machine. Up to the present time, the trial at Liverpool Street Station has the drawback of the gas being used at the same time. Now, gas and electricity do not do together; and I have such confidence in the electric light, that if I had the authority to do so, I would say either turn out your gas or I will turn out the electric light.

The cost is about ½d. per hour per lamp, each lamp consuming about 1½ inches of carbon per hour, including waste.

Mr. CROMPTON: As I have to pass through Liverpool Street Station very frequently in an evening, on my way to and from my works, I can testify to the excellent character of the installation of the Brush electric light used there. I do not hesitate to say that it has agreeably surprised me to see so many (16) lamps burning steadily on one circuit. I shall watch with much interest the comparison of the economical results of this installation as compared with one that I am connected with—viz., at St. Enoch's Station, Glasgow—where a large space is lighted by six large lights, on six separate circuits, each having its own Gramme machine.

I must apologize for the bad behaviour of my own lamps during this evening; I have never seen any one of my lamps burn so badly before. I must account for it by the hurry in which I had to get the tackle together: the machines borrowed from one place, the engine from another. I had no time to properly speed the engine to the machines, or regulate the lamps. I must state that I borrowed one of the lamps from a foundry where it has burned 310 hours, and has never been observed to wink or flicker perceptibly, when burning on its own circuit.

In answer to the gentleman who asked how I obtain such a white light, I account for it by the shortness of the arc I burn. When the arc is short, the major part of the light comes from the incandescent positive carbon, and these rays are intensely white. A longer arc gives more arc rays, and these arc rays are rich in violet and purple rays.

If I wished to provide an arc light for reading purposes, I should provide a dynamo-machine, with the bobbins wound so as to give low internal resistance; with the current from such a machine, I could burn an exceedingly short arc, without noise, and without damage to the machine by heating of the bobbin.

One gentleman has called my lamps highly-finished, expensive lamps, for special uses. This is not the case. All those I have hitherto made have been for the roughest use amongst contractors and for factories, to which class of installation I have hitherto confined myself.

I have not succeeded, nor do I think has Mr. Siemens, in producing a perfectly satisfactory arc light, for lighting reading rooms. The slight variations of intensity (independent of unsteadiness or flickering) are very trying to persons who have weak sight, and I am told that the light in the British Museum will never become thoroughly popular until this defect is remedied.

Mr. J. N. Shoolbred said that, notwithstanding the many and very ingenious improvements made by Messrs. Siemens during the past two years in the various apparatus for the production of the electric light, the largest share of the ameliorated condition and steadiness of the lighting as now effected by them would appear to be due, not so much to improvements in the lamps as to that of the machines. Especially, where several of the latter are used, to the intercommunication established between them, and to the use of an exciter, common to them all. This arrangement being, in fact, an approach in some sort to that adopted among the several parts of alternate-current machines; where the several parts are treated somewhat as the parts or elements of a chemical battery.

Undoubtedly, as stated by Mr. A. Siemens, and also by Mr. Cooke, the electric lamp is at present far more open to improvement than is the generating machine. Still, apart from the marked progress made by the very ingenious forms of lamp described in the paper, a very notable step in advance had been gained in those of Brush, of Werdermann, of Reynier, and of André. For these had solved, for continuous-current machines, the problem of the maintenance of a number of lights on one circuit; an advantage which so far, in practice at least, had been connected only with alternate-current machines. These lamps had therefore caused the term "single-light machines," as applied hitherto to continuous-current ones, such as the Siemens and the Gramme, to become an obsolete expression.

The labours of Mr. Crompton and of Mr. Heinricks, with lamps for a larger class of lights, afforded also a decided improvement, and in the direction where apparently most needed: simplification, and diminution of the weight to be set in motion.

Mr. Andrews had brought before the meeting two forms of

burner, with continuous currents, which bore some analogy to two forms of "candle" used in France with alternate currents. The plate lamp of Mr. Andrews, with its intermediate plate, insulated and placed in between the other two, resembles a number of De Meriten's candles placed alongside of each other; such as were shown by Professor Tyndall, F.R.S., in his lecture on the Electric Light, given at the Royal Institution in January, 1879. In this form of candle, one and sometimes two intermediate and insulated sticks of carbon occur between the electrodes; thus breaking up the otherwise rather long voltaic arc into others of lesser dimensions, by means of the induced poles caused at the intermediate carbons.

In describing his disc lamp, Mr. Andrews spoke of the rotation of the flame round the outer edge of the horizontal disc. There is in this lamp a spiral, placed vertically at a short distance above the disc; and probably this rotation of the flame may be in response to the circulation of the current round the coils of the spiral. The possibility of thus inducing a direction to the flame, as demonstrated by Dr. Moncel, has been taken advantage of in the design of the "Jamin" candle. In this form of burner, the current, after leaving one of the electrodes, is made to circulate four or five times vertically in the plane which is common to the two electrodes. The desired effect is, to cause steadiness in maintaining the voltaic arc at the extremity of the carbon rods. This burner in its early form had both carbons fixed; like a Jablochkoff candle, without the intervening insulating substance. At present, a small amount of play is allowed to one of the carbon rods. A spring causes this to fall against its fellow, when no current is passing; thus providing the contact necessary for lighting. While, when the current is passing through the spiral wires, a small armature, fixed on the sheath of the encircling-coils of the circuit-wire just referred to, is attracted to them, and in so doing forces away the carbon just referred to by means of a small hook projecting from it, and so effects the necessary separation. A rack and pinion urges, either upward or downward, the holder which contains both carbons; thus allowing a considerable length of carbon rod to be used, and at the same time retaining the luminous point at the same level.

In Mr. André's lamp, the addition of the water-joint socket to the glass vessel enclosing the light will prove useful in rooms, and other places where this moderate-sized light is likely to be used, even in cases where a complete vacuum is not an absolute necessity. For it has been shown by the researches of Mr. F. J. Evans, of the Gas Light and Coke Co., and also by those of Professor Dewar, F.R.S., that the amount of nitrous acid given off, as well as the consumption of the carbon, may be varied almost at will, according to the draught of fresh air which is permitted to have access to the light. The results of the former gentleman's experiments, with a Siemens small-sized machine and lamp to correspond, showed that in an hour's burning there could be given off from one and a half to five grains of nitrous acid, and from 98 to 178 grains of carbonic acid, according to the amount of air allowed; while Professor Dewar, with a similar machine and lamp, caused the amount of nitrous acid to vary from one to eight grains during the same period of burning.

With the financial results given at the end of the paper, the writer is unable to agree. Too much stress is laid upon the absolute amount of light produced at a given centre. The commercial problem is not, how much *light* can be produced for so much money, but how much *space* can be illuminated at that cost. If the latter test were applied to the three examples of illumination given in the paper, even allowing a somewhat higher standard of lighting than that generally in use, the results would probably differ from those given in the paper. The use of electricity as an illuminant implies a more generous standard of lighting than has hitherto been the case with the ordinary gas burner, though perhaps not necessarily so high a one as would be provided within a moderate distance of the light produced from a "medium" Siemens machine. The desire on the part of the public for a more abundant standard of illumination than hitherto, is shown by the very extended application which is being given to Sugg's new gas lamps, of one hundred and even of two hundred candle-power. They are thus proving themselves excellent forerunners for the general adoption of electric lighting.

Mr. Barrett, of the British Museum, stated that the figures

of cost given by Mr. Siemens were the actual amounts expended, and further remarked that the light had given every satisfaction since its first introduction, and that if the lights were turned on during daylight the students did not notice when daylight passed away.

Mr. A. Siemens, in reply to the remarks on his paper, said: I have had some difficulty in making my paper to everybody's liking. Some people blame me for not mentioning other makers' inventions at all, and some for mentioning their improvements, but not doing them justice. Anticipating some such feelings, I resolved in getting out my paper to describe only those improvements which had been introduced by Messrs. Siemens, with which I was familiar, and did not intend the paper to be comprehensive. I am pleased to hear of the success of the Jablochkoff light, and to have heard it acknowledged during the discussion that the Société Générale has really made the first important practical application of electric lighting, and, by thus drawing the attention of the general public to the matter, have promoted the cause of electric lighting. The only part of Mr. Berly's letter, perhaps, that I can take exception to, is that which says that the original 20 horse-power engine at Charing Cross has not been changed. This is stating the nominal power of the engine, whereas comparisons can only be made when the indicated power is given.

In reply to Mr. Hedges, whether the cost of the actual gas replaced at Blackpool by the six electric lights was given, I say, certainly not; because the four lights along the promenade there light up a length of about a thousand yards, which is ordinarily lighted by 40 gas burners, at a cost of about 10d. per hour—the four lights costing between 4s. and 5s. per hour. Mr. Hedges also complained of the dark spots. It is true that these do exist, and it is from the fact that when the lights were erected, the exact space covered by a single lamp at such a height had not been ascertained. To do away with the dark spaces, additional lamps will be fixed when the electric lighting is resumed.

The white colour is produced, as stated by Mr. Crompton, by burning a very small arc.

The improvements in Mr. Werdermann's lamps that have been

pointed out are very great, and have produced a remarkably high candle-power per horse-power expended for an incandescent light, being 700 against 50, which I read in a German newspaper as being the power obtained in an Edison lamp.

The electric light at present can only supersede gas, in point of economy, when used in large spaces, and the 87 per cent. gain over gas can only be effected where a light of 6,000 candle-power is really wanted—as, for instance, in the Albert Hall. In Messrs. Siemens' works, two electric lights costing 3d. per hour have superseded 30 gas lights costing 6d. per hour. The machines are driven from the general shaft, and are attended to by the boys in the shop, so that there is no charge for power or attendance, the only charge being for oil and carbons, amounting to 3d. per hour. In all cases, except where a powerful light is required in a large space, it is my opinion that gas must remain the illuminating agent for some time to come.

As to the method of measuring the candle-power adopted by Messrs. Siemens, to which Mr. Ladd alluded, I may say that ever since the report of the Trinity Board was published, Messrs. Siemens have always measured their lights in a horizontal direction, as Mr. Ladd states is done with the Brush light.

The President: Gentlemen,—I am quite sure that we are all deeply indebted to Mr. Alexander Siemens for his paper, and for the enjoyable discussion which it has produced.

As regards the ground covered by the paper, I am bound to say that before it was even written, Mr. Siemens objected very strongly to making a public exhibition, in fear of failure through want of preparation and trial; and it was only at my strong personal request that the exhibition we have had was displayed.

The history of the progress of the electric light, up to the present time, has not been a very brilliant one. It has not made a very remarkable advance during the past twelve years; nevertheless, the advance has been such that the British Museum, *Times* office, and many other places have decided to permanently adopt the electric light, and therefore its permanent success is secured.

The evidence before us, however, has shown that there is still vast room for improvement. Lamps will blink and lamps will

wink, for which various excuses are made: sometimes it is bad carbon, and sometimes it is the poor unfortunate engine, that does not get half enough credit given to it. The engine is scarcely ever spoken of; but my impression is that one, if not the greatest of the successes of the many applications of the electric light to public purposes is due to the engines as much as to the lamps; and though the lighting to-night has not been a success, I cannot think it is due to the engine.

There is great room for improvement in the electric light, and plenty of scope on which the members of the Society can turn their energy during the next twelve months to improve the carbon or lamp; the Siemens and Gramme machines are as near perfect as may be. Opinion in favour of either of these machines over the other is about equally divided; and Dr. Hopkinson having proved that 90 per cent. of the speed is utilised, it is quite evident that there is no room for improvement left, even for inventors on the other side of the Atlantic.

We hear a good deal at times of the comparative cost of the electric light and of gas. Properly speaking, the two lights are not to be compared. Some say—Here is a light of 6,000 candle-power at 3d. an hour: if it were produced by gas, it would cost £3 an hour. Such a statement reminds me of Mr. Dick's reply to the statement that "his room was not big enough to swing a cat in,"—he said, "I don't want to swing a cat. I never do swing a cat." So with an electric light of high power. It is not wanted in a place where small light power is required. And I agree with Mr. Shoolbred, that the amount of space illuminated is the proper measure of the value of the illuminator. When a definite basis or system can be arrived at by which it can be said that gas will illuminate say 400 square feet at so much per hour, and the electric light will illuminate the same space at so much per hour, then it will be possible to make a fair comparison between gas and electricity.

I find that, as far back as 1861, Faraday said: "And this gives one a strange sensation as to what may be going on in a gas flame or a fire. Let us hope that some day we may transfer this light and heat, and all their other powers, to a distance, and use

them at pleasure; laying on, not gas, but the powers of the gas or oil, and so having a lamp more wonderful than Aladdin's."

Gentlemen,—We have a lamp more wonderful than Aladdin's in many respects, for neither he nor anyone else ever succeeded, until the present generation, in producing after all a light that is superior to daylight.

As to the term "candle-power," used for calculating the value of the electric light, I can only say that the electric profession generally would be very grateful if the term could be got rid of altogether. It is a nasty word, and of none too definite a meaning. Some scientific term, based on the amount of space illuminated, is desirable; and if the two gentlemen present (Professors Ayrton and Perry) who have produced a capital new photometer, will turn their attention to the proper designation of this unit of light measurement, we shall all be relieved of a term which now we cannot understand.

The Meeting then adjourned until Wednesday, April 14th.

The Eighty-seventh Ordinary General Meeting of the Society was held on Wednesday evening, April 14th, 1880, at the Institution of Civil Engineers, 25, Great George Street, Westminster —Mr. W. H. PREECE, President, in the chair.

The minutes of last meeting were read, and the names of new candidates announced.

The PRESIDENT: Last meeting I announced the fact that Dr. Siemens, F.R.S., had very kindly promised us a paper on the recent advances made by him in the practical application of the dynamo-machine to the fusion of metals, and for promoting vegetation. Dr. Siemens has requested me to postpone fixing a date for the paper for a week or so, as, by then, he will have made further experiments, and will be able to bring the completed results before this Society up to the latest date. I hope to announce the actual date next meeting.

The SECRETARY then read the following paper—

## NOTE ON SOME EFFECTS PRODUCED BY THE IMMERSION OF STEEL AND IRON WIRES IN ACIDULATED WATER.*

By Professor D. E. HUGHES, Member.

During a discussion upon a very interesting Paper by our President, "On the Durability of some Iron Wire," I mentioned a fact which I had lately observed, *and which must have been*

* Since the reading of this paper two very valuable communications on this subject have been brought to the author's notice, entitled "On the Influence of Acids on Iron and Steel," by William H. Johnson, B. Sc. (*Manchester Literary and Philosophical Magazine*, March 1874); "On the Action of Hydrogen and Acids on Iron and Steel," by the same author (*Proceedings of the Royal Society*, No. 158, 1875).

These papers clearly show that iron and steel are rendered brittle by acids, and that its textile strength is also affected.

Mr. W. H. Johnson's researches were partially anticipated by M. Cailletet, who, in 1868 (*Comptes Rendus*, Vol. LXVI., No. 16, p. 847) describes the action of acids on iron and steel, their absorption of hydrogen, and their brittleness in consequence.

*observed by many others*, viz., that steel or iron wires immersed for a few minutes in acidulated water, containing $\frac{1}{10}$th sulphuric acid, became excessively brittle.

Our President has since kindly asked me to make a few more experiments on this subject, and to embody them in the form of the present Note.

Upon repetition of these experiments, I have found that this brittleness is no mere accidental result, due to some flaw in the steel or iron wires, but that the resulting brittleness is invariable in all kinds of steel as well as iron. Nor is the effect due to any specific proportions of sulphuric acid to the water: nor, in fact, as we shall see later, to any particular acid. The effects, however, seem confined to steel and iron; as by similar treatment I have as yet obtained no perceptible effect on copper or brass.

At first I was inclined to believe that the effects were due primarily to a change in the molecular structure, but a more extended series of experiments has led me to adopt entirely the view taken by my friend Mr. W. Chandler Roberts, who pointed out that the effects were most probably due to the absorption of hydrogen.

I have tested these wires in my Induction Balance, but can find no change whatever in their magnetic conductivity, nor any change which would be the equivalent of those produced by heat, strain, torsion, or tempering; but there are very evident results produced, if the conditions of the experiments are such as to favour the absorption of hydrogen. For instance, if we reduce the proportion of sulphuric acid to $\frac{1}{20}$, we find that it requires some 30 minutes' immersion to produce the full effect, a few minutes' immersion producing no perceptible result. If now, we place an amalgamated zinc plate in the same liquid, and join the two extremities, we have an ordinary battery where hydrogen is given off on the steel wire. Now, as the production of the hydrogen by the decomposition of the water is much more rapid than before, we find that a few minutes' immersion produces a far more brittle wire than could be obtained by hours of simple immersion, and we have the result free from any doubt as to its being a mere surface action, for, if we immerse *the wire* alone, surface corrosion rapidly takes place, but by

simply connecting it with the zinc the steel is perfectly protected, retaining its original bright surface, for any time, as long as it is so protected.

It is not absolutely necessary that we should join the zinc in the same cell, for, if we pass a current from a few cells of an external battery through two steel wires as electrodes in sulphuric acid and water, we find that both wires have become brittle, though in a very different degree, the wire connected with the zinc or negative pole remaining bright, although excessively brittle, whilst the one connected with the positive pole is much corroded, and but feebly brittle, with this arrangement. I find that sulphuric acid is no longer required, but that all acids, neutral salts, and ordinary water, produce an active effect, the time required being simply inversely as the conductivity of the liquids employed. When water or most neutral salts are used, we find the negative pole quite bright but brittle, and the positive pole much corroded, but not at all changed as regards its flexibility.

I believe that these effects are due to the absorption of hydrogen when the hydrogen is in the "nascent" state, for I have obtained no results by the continued immersion of the wire in carburetted hydrogen gas (ordinary lighting gas), but when plunged into a medium containing the hydrogen just freed from its combination, its effects are most remarkable: for, if we immerse a wire into sulphuric acid and water, say $\frac{1}{20}$, the effects are slow, requiring at least 30 minutes; but if we let fall into this water some scraps of zinc, hydrogen is rapidly given out, and by now immersing the steel wire in this gaseous liquid, taking care not to touch the zinc, we find that the steel becomes rapidly brittle, whilst its surface is free from corrosion, due no doubt to the protecting surface of the surrounding hydrogen.

Hydrogen seems to permeate through the entire mass, for iron rods $\frac{1}{4}$ inch thick were equally affected, requiring more time, or in other words, a supply of nascent hydrogen sufficient for the larger mass; and once the wire has become hydrogenised (if we may be allowed the expression), it retains it under all circumstances of time and change of surrounding atmosphere: heat alone, of all the means I have tried, has any power in removing this effect; and if

we heat a wire to cherry-red in a spirit lamp, we find that it is completely restored to its primitive flexibility in a few seconds. This same wire, however, on being immersed in the acidulated water, rapidly becomes again brittle; we may thus at will render the same wire flexible by previously heating it, or render it exceedingly brittle by favouring its absorption of hydrogen.

I have remarked that a wire immersed in sulphuric acid and water in any proportion, say $\frac{1}{18}$th, becomes more electro-negative than at the first instant of plunging. If we take amalgamated zinc as the positive element, and a steel or iron rod or wire for negative, we find that there is such a remarkable similarity of electro-motive force between all kinds of steel and iron, that we are forced to the conclusion that we are simply testing the electro-negative qualities of hydrogenised iron; the force being with amalgamated zinc ·56 volt.

I noticed here a remarkable fact, and which does not agree with the results of many authorities. I found that as soon as the iron rod had absorbed its maximum of hydrogen (a few seconds after being short circuited), that it produced a constant cell, giving but small traces of polarization when or after being short circuited for hours at a time. There occurs, however, a slight diminution of electro-motive force after a few days' hard work, being then ·52, due to the acidulated water becoming more neutral by the formation of sulphate of zinc and iron. If, however, we wish to restore its full electro-motive force, we have only to short circuit the cell for a few seconds, torrents of hydrogen will be given off, and its electro-motive force becomes, on testing, of its highest value, ·56.

If we short circuit the hydrogenised iron cell for one minute, and at once test its electro-motive force, we shall find at the first instant a certain amount of polarization, about 10 per cent., but the cell rapidly recovers, being at its full initial force in ten seconds' repose; whilst cells with carbon, platinum, and all other negatives yet tried, did not recover their polarization in one minute's repose.

Taking the Smee battery as the best example of depolarization in a single liquid, and comparing the constancy of this cell with that of the hydrogenised iron, I find, that according to Mr. Latimer Clark's experiments, in his work on electrical measurements,

that the electro-motive force of a Smee cell is 1·017, but when in action only ·446. Thus its electro-motive force in action is less than that of the iron cell, and its polarization some five times greater than that of iron.

I have submitted these results (rather hastily obtained) to our President, Mr. W. H. Preece, and he has kindly consented to have some exact measurements made of the electro-motive force of hydrogenised iron, and its comparative freedom from polarization with all other metals employed as negative elements in a single liquid cell, and I hope this evening we shall hear the results.

A practical application of iron as a negative may be mentioned. If we wish to purify mercury from any zinc, or any metal less negative than iron, we have only to place the mercury in dilute sulphuric acid, and then introduce an iron rod so that its lower portion shall make contact with the mercury, hydrogen is now freely and constantly given off by the iron, and this continues until all traces of zinc have disappeared; and as a proof of this, if after a certain time, when no hydrogen is given off, we simply touch the mercury with zinc for an instant, the hydrogen at once reappears, and continues until this small portion of dissolved zinc has been separated from the mercury.

In order to render evident the remarkable depolarizing power of iron, we use in the same cell several negatives, such as carbon, platinum, silver, copper, and iron; and if we test these negatives separately for its initial electro-motive force, we shall find them all superior to iron; but if we join all the negatives together, and short circuit the whole with the zinc, iron alone will freely give off its hydrogen, whilst carbon will appear to be entirely inert, and if after this short circuiting we insulate or separate the different negatives, we shall find on testing them that they are all polarized, carbon being the most so, and iron comparatively quite free, and giving at the moment of insulation the highest electro-motive force.

In conclusion I may add, that if hydrogen seems to be an enemy of iron and steel, rendering it brittle, on the other hand, it is perhaps its best friend in rendering it more negative, and whilst under its entire influence, completely preserving it from oxidation or rust.

The PRESIDENT: Before inviting discussion on Professor Hughes' paper I will ask Mr. Chandler Roberts to communicate a few remarks on the same subject, which he has kindly offered to make.

Mr. CHANDLER ROBERTS: Professor Hughes' observations interested me very much, as they remind me of experiments I made more than ten years ago, in conjunction with the late Mr. Graham, which showed that nearly all metals absorbed gases to a greater or less extent; each metal seeming to select its own peculiar gas, palladium, for instance, occluding no less than 900 times its volume of hydrogen. The method consisted in heating a metal in an atmosphere of the gas in which it was allowed to cool. In other cases hydrogen, evolved by the action of a battery, was allowed to act on the metal under examination, and its power of absorption was measured by subsequently extracting the gas by heating the metal in vacuo. On the table I have the original apparatus by which certain of the experiments were made. It consists of a long glass tube containing acidulated water in which two palladium wires are immersed, and which are in connection with the cells of a battery. When contact with the battery is made oxygen is given off in the glass tube from the one wire and hydrogen is absorbed by the other wire, and the absorption of hydrogen will manifest itself by a falling of the long index attached to the glass tube, caused by linear expansion of the wire.

When Professor Hughes told me of the facts he relates in his paper it occurred to me that the wires occluded hydrogen much in the same way as palladium does, and by experiment I found that on heating the steel wires in vacuo it is possible to remove from them at least ten times their volume of hydrogen, the latter being quite pure and not contaminated with hydro-carbons, provided care is taken to extract any natural gas occluded by the wire during the metallurgical process involved in the manufacture. (The palladium wires in the glass tube were connected with the battery, and the pointer soon began to fall.) The falling of the index is caused by the expansion of the wire which absorbs hydrogen; and on reversing the battery the pointer moves upward by the contrary action taking place. I have not yet established the fact of a change in volume of steel wires 300 mm. long when hydrogen either

enters or leaves them, but it is, of course, possible to measure any change of volume which the metal may sustain by the removal of the occluded gas.*

The absorption of hydrogen by iron has also been noticed by M. Cailletet, who showed that thin plates of iron are penetrated by the gas, although the effect may have been partly due to the penetration of the dilute acid in which the metal was placed.

The PRESIDENT: I am sure we are very much indebted to Mr. Chandler Roberts for favouring us with the remarks he has made. I would like to ask him whether he can tell us that the absorption of hydrogen which he has proved to take place in the case of iron can be adduced as the cause of the brittleness shown by Professor Hughes to exist.

Mr. CHANDLER ROBERTS: Professor Abel can perhaps better answer the question. It is, no doubt, a fact that iron becomes crystalline after it has occluded the gas: and it seems to me possible that while the hydrogen is entering, the iron may take a mean advantage of the opportunity of rearranging its particles, the result being the remarkable brittleness to which Professor Hughes has called attention.

Professor ABEL, C.B.: I must confess to ignorance upon the question raised. Professor Hughes' observations have excited much interest in my mind, and their having only recently been made is a striking illustration of the blindness of workers. I myself was engaged years ago, making a number of experiments on the immersion of short pieces of iron and steel wire in dilute acids, but it was for the purpose of studying the structure of the wires as bearing upon an important manufacturing question, and I confess that the particular effects which Mr. Hughes has described as being produced by such immersion escaped my notice altogether. I would like to ask Mr. Chandler Roberts if he has measured the volume of gas which he was enabled to separate

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* The fact escaped my memory that Mr. W. H. Johnson stated, in his valuable paper published in 1875 (*Proc. Roy. Soc.*, Vol. XXIII., p. 179) that he had detected a very small change in the length of annealed iron wire as the result of the occlusion of hydrogen.

from the iron in his experiment before it was treated with hydrogen.

Mr. CHANDLER ROBERTS: The amount of natural gas varies from three to ten times a volume.

Professor ABEL: One would hardly imagine that the brittleness developed in the metal could be due simply to the absorption of the additional ten volumes of gas.

The absorption of hydrogen by iron and steel wires is very much less than in the case of palladium, where it amounts to about 200 times the volume of the metal. It appears to me that we can only conceive that the change in the physical character of the metal must be ascribable to some such action as hinted at by Mr. Roberts, *i e*, some kind of molecular disturbance of the iron by the entrance of the gas into the mass. At present, we are seeking for an explanation without any adequate or sound foundation to work upon. I can conceive that some such action as suggested may take place; we have instances of molecular changes in iron itself, which may be brought about by causes which must, I think, be regarded as less disturbing in their character than this entrance and exit of gases into and from the metal. One would like to hear what the views of one familiar not only with the scientific but also with practical aspects of these questions are. I know of no one more competent than Mr. Anderson to express an opinion on the cause of this physical peculiarity of iron, and I beg that the question may be transferred to him.

The PRESIDENT: Before discussing the matter further, I may mention that Mr. Stroh has made some experiments with wire that may assist Mr. Anderson to form some opinion; therefore I will ask Mr. Stroh to kindly relate his experiments.

Mr. STROH: When Professor Hughes first brought the question before the Society, I felt doubtful as to how far the brittleness penetrated the wire after immersion in acidulated water. I took a steel wire about $\frac{1}{8}$th inch thick, and found that brittleness was produced after immersion in dilute sulphuric acid for about half a minute. I filed the surface, repolished the wire, and still found it to be brittle; and, after making the wire much thinner by repeating this process, the brittleness continued, and there is no

doubt that it is not superficial, but penetrates to the centre of the wire.

I was anxious also to find out other evidences of change produced by similar immersion, but was unable to find anything beyond the brittleness.

At the suggestion of our worthy President, I tried experiments to ascertain the tensile strength of wire after immersion in acidulated water, and I found that in this respect wire remained unchanged as compared with its natural condition. I have tried iron, steel, hard-drawn wire, and annealed wire, but in all cases the ordinary tensile strength remained unaltered.

Heating restores the flexibility of wire so made brittle; but, of course, in the case of hard-drawn wire, the hardness is destroyed also.

Mr. ANDERSON: When the President invited me to attend this meeting and hear what Professor Hughes was going to say on the occlusion of hydrogen in iron and steel wire, I at once thought that I might hear of something to my advantage, because I have the honour of presiding over the Committee of Research of the Institution of Mechanical Engineers on the hardening and tempering of steel. The question at present before that Committee is the explanation of the hardening and tempering of steel; and the theory, for which I believe I am responsible, but which I think also finds favour with my colleagues, is that it is due to the greater or less quantity of hydrogen contained in the steel tending either to separate the particles or to allow them approach to more closely together. The experiments Professor Hughes has described confirm this theory, and show that the excessive occlusion of gases tends to separate the particles to such an extent as to make the steel more brittle. The application of heat again expels the gases, and allows the metal to return to its normal state; but if suddenly quenched, so as to prevent the gases re-entering to some extent before the steel is cool, its particles are able to approach closer together, and therefore become more compact and render the steel harder. I should like to ask Professor Hughes whether, in the brittleness he has noticed in steel, he has found its hardness increased or otherwise?

Professor HUGHES: I have not been able to verify that fact.

The PRESIDENT: Mr. Stroh will perhaps answer.

Mr. STROH: I think I can safely say that the hardness of the steel is not affected in the least degree.

Mr. ANDERSON (continuing): When a theory has been started, it is important to become acquainted with everything in the way of trustworthy experiment that may throw light on the subject. Professor Hughes' experiments show that the occlusion of hydrogen gas tends to separate the particles of steel, and this separation tends to reduce their cohesive power, according to the well-known laws of attraction. It is also known that hard steel carries a greater strain per square inch than the same steel softened, simply because the particles are very close together, and their cohesive force thereby increased. The first idea of this theory was suggested to my mind by Edison's experiments in searching for a permanent substance for his electric light produced by the incandescence of wire. He found that platinum fell to pieces, and propounded the theory that this was caused by the escape of occluded gases causing cracks in the platinum and gradually destroying it. Edison argued that if, by repeatedly heating the platinum in vacuo, he could get all the gases out, he would then obtain a permanent material. He appears to have performed the experiment, and arrived at the result that an exceedingly hard and permanent substance was capable of being produced

Professor ABEL: I think the case alluded to by Mr. Anderson does not quite correspond with that established by Professor Hughes' experiments, which show that iron wires may be made brittle and restored any number of times; whereas Edison's experiments appear to have soon brought platinum to a state of rest as regards the alteration of its molecular structure.

Professor HUGHES: Yes.

Professor ABEL (continuing): Another important point to bear in mind is that the increase in volume of gas absorbed is only about double that originally existing in the metal. Mr. Roberts finds about 10 volumes of gas existing ordinarily in malleable iron; the maximum volume of hydrogen absorbed, and by which that gas is displaced, is therefore only double that original volume.

Is it possible that the absorption of this additional volume of gas is sufficient to account for the supposed separation of the molecules and great change in the physical qualities of the metal?

It appears to me that there is still something wanting to explain the great change which the metal undergoes, and the restoration of its original properties for an indefinite number of times.

Professor HUGHES: During my experiments I continually kept in mind the theory advanced by Mr. Anderson, and as yet have found no experimental fact which could support it; on the contrary, I have remarked that tempered steel occludes hydrogen with as much if not more facility than soft steel, and does not become soft after having done so. I have not, however, made a special study of this point, but will do so at the earliest opportunity.

The action discovered is very peculiar in its behaviour. There is no doubt that steel or iron becomes excessively brittle, and breaks off like glass and appears rotten, that it can be restored by heat and rendered brittle by acid many times. But the most curious fact is that if we take a wire, when brittle, and strain it, expecting it to break to pieces, it is almost as strong as before. This surprised me, and I tested the strained wire for brittleness, but found it had disappeared, and I concluded that the mere strain had had the effect of excluding the hydrogen. The departure of the hydrogen, of course, cannot be witnessed, but in practice the strain seems to rearrange the particles, and this molecular rearrangement seems to allow the hydrogen to escape.

Professor W. GRYLLS ADAMS: The experiments which have been described this evening in Professor Hughes' paper and those by Mr. Roberts all show that the increased brittleness of iron and steel is accompanied by an expansion of the iron and steel rods. In the case of palladium also, the occlusion of hydrogen is accompanied by an expansion of the rod of palladium. In these cases, the spaces between the molecules of the metal would seem to become occupied with occluded gas to such an extent that those molecules are driven farther asunder, and thus the force of cohesion between them is diminished. Mr. Stroh's experiments

point in the same direction, and show that the separation between the molecules is not confined to the surface of the rod, but takes place throughout the whole mass. All these experiments seem to support Mr. Anderson's theory that the brittleness is due to the diminution of the force of cohesion arising from the increased distance of the molecules from one another, when the interspaces are occupied by the molecules of other substances such as occluded hydrogen. Possibly, some facts connected with the melting of metals and their alloys have a bearing on this question. The molecules of an alloy of two or more metals are more readily separated than the molecules of the simple metals, as shown by the fact that the melting point of an alloy is lower than that of the metals; and the greater the number of metals in the alloy, the lower is its melting point. In this case, it would seem that the molecules of the two or more metals scattered throughout the whole mass, and bound together in a way which we do not yet clearly understand, so affect the mass that the force of cohesion of the molecules of each metal is diminished, and the melting point is reached sooner in the case of alloys than in the case of the solids of which they are made up.

Mr. WILLOUGHBY SMITH: In the early days of submarine telegraphy, the joints in the gutta percha covered copper conductor were made with silver solder, borax being used as a flux. But the great heat necessary to make such a joint softened the gutta percha for a considerable distance along the wire, which made the process of recovering the conductor with gutta percha difficult. Consequently, a soft solder, with chloride of zinc as a flux, was used; but if the chloride of zinc was not properly prepared, which was frequently the case, the free acid affected the copper to such an extent that in time it became quite rotten. I therefore am surprised to find that Professor Hughes has not included copper in the list of metals mentioned in his paper.

The joints in the iron strands which formed the covering of the 1858 Atlantic Cable were made in a similar way, and were affected the same by the free acid.

Professor PERRY: I do not think that sufficient notice has been taken of the result of Mr. Stroh's experiments, that the strength

of the iron was not diminished. This result can hardly be taken as quite certain, since it requires very careful experimenting upon indeed; but if it is the fact, it is very difficult to see how brittleness can be explained as due to loss of cohesion, from gas-separation of molecules, whilst the tensile strength remains unchanged. We ought, I think, to discourage vague explanations of this kind. The same thing might be said as to Professor Adams' analogy with alloys. I think it is the fact that some alloys are much stronger than their components.

Mr. CHANDLER ROBERTS: An alloy of tin and copper is much stronger than either of the substances composing it.

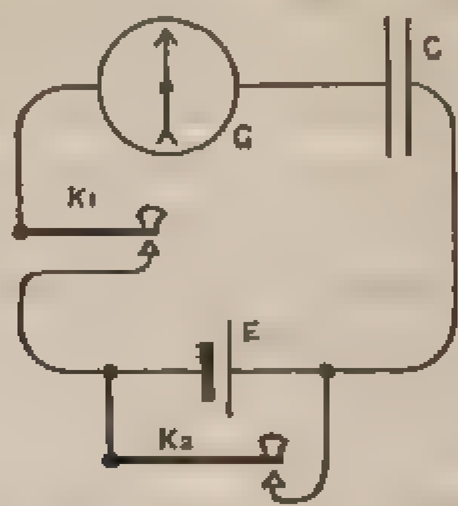
Professor PERRY (continuing): As we have so much chemical talent here to-night, might I ask whether there is any substance, a conductor of electricity, which does not occlude hydrogen? This is a very important point on the electrical side of the case, because it is this occlusion of hydrogen in polarization experiments which stops Mr. Ayrton and myself in our investigation of the analogy between polarization in ordinary dielectrics and that in the voltameter, and we should very much like to make experiments with a metal, unlike platinum, which does not absorb hydrogen.

Mr. CHANDLER ROBERTS: So far as I remember, silver does not occlude hydrogen, or at any rate not in quantities exceeding two-tenths of the volume of the metal. It takes up about four or five times its volume of oxygen, but I think not hydrogen. Probably the brittleness may be due to the removal of dioxide of copper, a certain amount of which is necessary to the ductility of copper.

The PRESIDENT: The discussion having now turned upon polarization, I think it is opportune to ask Mr. Kempe to favour us with the results of some experiments on the point, which he has made at the suggestion of Professor Hughes.

Mr. H. R. KEMPE: The experiments I have made at Professor Hughes' request bear chiefly upon the polarization of different metals when formed into couples, with zinc as a po[illegible]
M[illegible] time for making the experiments w[illegible]
results, therefore, must not be taken as

My method of experimenting was as follows: A Thomson galvanometer and a condenser were joined up in the circuit of the galvanic couple, the polarization of which it was desired to determine, two keys being included in the circuit. The following sketch shows the arrangement:




$K_1$, $K_2$ are the two keys, the latter of which, by being depressed, short circuited the battery. By depressing $K_1$, leaving $K_2$ open, the inrush of the current into the condenser C produced a throw on the galvanometer G, which represented the electromotive force of the battery E. Now, if $K_1$ were kept depressed, and then $K_2$ pressed down, an equal but reverse throw to the foregoing was obtained. Again, if $K_2$ was kept depressed for a definite period, and then raised ($K_1$ still being held down), a throw in a similar direction to that observed on depressing $K_1$ would be noticed; and if the throw were equal to that first obtained, it is evident that no polarization can have taken place; in actual practice a diminution is observed, the amount of which represents the degree of polarization which has taken place in E. If, after $K_2$ had been pressed down for a definite period, it was then released, $K_1$ having been previously freed, then on again depressing $K_1$ and observing the throw obtained, the amount of the latter would indicate the degree of recovery which the cell had undergone.

The cell taken as a standard was an ordinary Daniell, with circular zinc and copper elements.

The electromotive force of this cell, represented by the throw obtained on depressing $K_1$, was equal to 190; this value I took as representing the initial figure 1.

On joining up other couples and taking their electromotive forces, without allowing any time for polarization, I got the following results:

| | | | |
|---|---|---|---|
| Ordinary Daniell couple | ... | = 1·00 | |
| Zinc-Carbon ,, | ... | = 1·21 | very variable. |
| ,, Platinum ,, | ... | = 1·05 | ,, ,, |
| Daniell, with sulphuric acid | ... | = 1·01 | |
| Zinc-Copper couple | ... | = ·95 | very variable. |
| ,, Iron ,, ... | ... | = ·56 | |
| ,, Amalgamated copper | ... | = ·16 | |

After the couples had been on short circuit for one minute, and tested for polarization, the results came out as follows:—

| | | | |
|---|---|---|---|
| Daniell, charged with zinc sulphate | fell ... | 9·7 | per cent. |
| ,, ,, sulphuric acid, | ,, ... | 9·8 | ,, |
| Zinc-Iron couple ... ... ... | ,, ... | 12·0 | ,, |
| ,, Copper ,, ... ... ... | ,, ... | 50·0 | ,, |
| ,, ,, amalgamated couple | ,, ... | 45·0 | ,, |
| ,, Carbon couple ... ... | ,, ... | 50 0 | ,, |
| ,, Platinum ,, ... ... | ,, ... | 51·0 | ,, |

In making the experiments, I noticed that in point of steadiness the zinc-iron couple was far ahead of all the others. It was not absolutely steady, as variations for different periods of time were noticeable, but on the short circuit being opened, the recovery of the couple was very rapid indeed: and it is possible that a mean or limit of variation may be arrived at by taking a sufficient time, but this point I did not determine.

The Daniell recovered itself more slowly, but, as in the case of the zinc-iron couple, did not reach its initial value within the limits of trial. The other elements showed much more variable results. The zinc-platinum, zinc-copper, and zinc-carbon would take a very long time to come back to their original electro-motive force after being on short circuit. In the zinc-iron couple the current was very steady, *i.e.*, the electro-motive force and resistance taken together varied very little as compared with other elements, and, therefore, no doubt the zinc-iron couple may prove a very useful form of element for making experiments in which

very steady currents are required. The couple is easily made. Its resistance is very low and gives a good, so called, quantity current, and possibly for that reason might prove useful for electro-lighting purposes.

The facts and figures I have mentioned are merely extracts of a large number obtained by the experiments, but what I have said represents a summary of the whole.

Mr. VON TREUENFELD : The subject of the discussion is of very great importance in telegraphy, especially with regard to the question, "Is galvanizing injurious to telegraph wire or not?" I had an occasion of learning the injurious effect of galvanizing iron wire as far back as ten years ago when constructing telegraph lines in South America. A large quantity of iron wire was ordered from England which was tested before being galvanized by a competent inspector, and proved satisfactory in accordance with the Government specification. It was afterwards galvanized and shipped, but on its arrival in South America it was again tested and failed to reach the given standard; after an investigation it was proved that it had suffered from exposure to acidulated water during the galvanizing process.

Mr. STROH : In answer to Professor Perry's question, I should like to add a few words. The experiments I tried on the tensile strength of the wire were perhaps not so sensitive as they might have been, and minute differences might exist between wires which had been immersed in acidulated water and wires which had not been so treated, but still I am quite certain that there existed no great or appreciable difference. I forgot to add that after a brittle wire had been subject to strain, its brittleness appeared to be reduced. It is difficult to make any accurate comparisons, because where a wire is once broken to ascertain its brittleness, its tensile strength cannot be again tested.

Professor AYRTON : Professor Hughes has found that copper does not, like iron, become brittle when immersed in dilute acid. The following fact, noticed some years ago by Professor Perry and myself when experimenting on the contact theory, may bear on this point. It is, of course, well known that the electromotive

force of a simple voltaic cell rapidly falls if the cell is employed to send a strong current; and the cause is also known, namely, the deposition of hydrogen gas on the copper plate and oxygen on the zinc. But the following is perhaps not so well known, namely, that the electromotive force of a new voltaic cell which is not employed to send a current steadily rises for some time; that is, if a zinc and copper plate be joined respectively to the electrodes of an electrometer, and then dipped into dilute sulphuric acid, the electrometer reading is found to steadily increase. Is this increase in electromotive force due to the fact that copper occludes oxygen from the liquid, and not hydrogen? If so, it may account for the copper not becoming brittle like the iron.

Mr. J. MUNRO: It has occurred to me that the fact of the wire becoming brittle, while retaining its tensile strength, might be due to a mechanical action. When a wire is bent, one side is in tension and the other is in compression. Now, the particles of gas might block up the inter-molecular spaces in the part under compression, and, by preventing the bending, might cause the brittleness. Also, when a wire is stretched the molecules of gas must, I think, escape; and so it is conceivable that by stretching a wire after it has been made brittle, it may be brought back to its original pliability, as Professor Hughes has found.

Professor ADAMS: Mr. Stroh's experiments on the stretching of wires seem to show that some of the occluded gases are expelled by the stretching. When a wire is stretched, its volume is slightly increased, but its diameter is diminished, and the lateral approach of the molecules of the metal towards one another will tend to drive out the occluded gas, and to render the metal less brittle.

Professor HUGHES: I have listened with very great pleasure to the discussion on my paper, but I do not feel very clear yet as to the theory on the point. There is the certain fact that the iron wire absorbs hydrogen and that the wire becomes brittle, but the reason why has not yet been given. But one very extraordinary fact has come out. I have always understood, and have been taught to believe by most electrical works, that iron is one of the

worst negatives that could be employed. Authors record iron as polarising rapidly, and this error has been copied and repeated in electrical works until the statement has become one of common belief. Now, if iron absorbs hydrogen, and hydrogen is the cause of polarization, one would expect to find greater polarization in iron than in any other substance; but, to my surprise, when tested (as also stated by Mr. Kempe) it was less than any known substance, and after short circuiting an iron and zinc element for hours it rises extremely rapidly again. Its constancy is higher than any other single liquid cell. In fact its constancy is so remarkable that it can only be compared with a Daniell. The resistance of this cell is also less than any other exposing the same amount of surface—in a similar acid. When we measure the resistance of a single liquid cell we are really not measuring the resistance of the liquid at all; for every negative we put in, all being of similar dimensions, there is a different resistance to that liquid, and it is really the resistance of the negative element and its polarization that varies. My view of the matter at present is that iron absorbs hydrogen, and that being full of hydrogen, when hydrogen comes against it in the cell, it is repelled and given off in the streaming torrents of bubbles that are so astounding. Really, the iron cell is worthy of being studied and investigated—as where large and constant currents are required, as in the case of electro-metallurgy, electric light, &c., it is the most simple and economical battery that we possess.

The PRESIDENT: We are very much indebted to Professor Hughes for having brought this subject before us, and also to Mr. Chandler Roberts and Mr. Stroh for supplementing it. The chief point brought out in the remarks is, really, how very ignorant we are of one of the very commonest things taking place round about us. Here is the mysterious substance, iron, which takes up such innumerable forms, which, with the addition of a little carbon, entirely changes its character, which becomes one moment soft and another moment brittle; which is used sometimes for the negative and sometimes for the positive pole of a battery, and which takes all these protean shapes without our having the slightest notion of why it is.

It shows how by pursuing a subject of this kind there are others besides Professor Hughes who can enlighten us in investigating the electrical behaviour of substances. There is a very interesting behaviour of iron bearing to a certain extent on this subject, but which is totally different in its character, and that is the passivity of iron. It was Sir John Herschell who found that under certain circumstances iron was in a totally negative state, that is, acids, nitric acid in particular, would not attack it. Schonbein re-discovered this, investigated it, and Faraday followed it up, and it led to iron being largely used for batteries. There was the historical Maynooth battery containing about two hundred square feet of iron, and nearly half that quantity of zinc which produced almost torrents of electricity. It fell out of use, I do not know why. But Professor Hughes has brought before us to-night such a fact that must force all electricians to investigate still further the behaviour of iron for batteries, and to see whether this curious material cannot be utilised for practical purposes to a greater extent than it hitherto has; and I am sure I am only re-echoing the opinion of everybody here present when I say that we are extremely indebted to him for bringing the paper before us.

Subsequently to the discussion Mr. R. S. Newall, F.R.S., wrote to the President:

"I thought that the fact that iron wire immersed in acidulated water becomes excessively brittle was well known to wire-drawers and users. I certainly have known it for the last forty years, and have used it to ascertain whether the wire was newly made or was brittle from bad material or other cause. An iron wire gets completely *saturated* with acid if it has been cleaned with it, or 'pickled' as it it technically called. To prove this, break the wire and wet it on the point of your tongue, and a bubble of hydrogen will be given off, the wire is as brittle as a carrot, but if it be put into stock and not used for a month or two the acid evaporates and wire which might have been condemned as bad by a person not aware of the fact, becomes quite tough and good."

The SECRETARY then read the following paper :—

# ON THE ADHESION OF METALS PRODUCED BY CURRENTS OF ELECTRICITY.

By A. Stroh, Member.

1. It is well known that when a circuit conveying a powerful current of electricity is made or broken, a spark is visible at the point of contact. The spark is most vivid, and is also accompanied by a snapping sound when the circuit is broken.

The intensity of these effects increases with the strength of the current flowing, until, in the language of Gassiot, it becomes "a vivid electrical flame discharge."

It varies with the metal employed in making contact, and it is always very striking where mercury contacts are used. The *colour* of the spark is known to depend upon the metal employed in making contact.

It is also well known that, under certain circumstances, contacts have a tendency to stick or adhere, as in the case of relays; and the object of the present Paper is to draw attention to certain facts in connection more especially with the *making* of contacts, which are thought to be of a novel character, and therefore of interest to this Society.

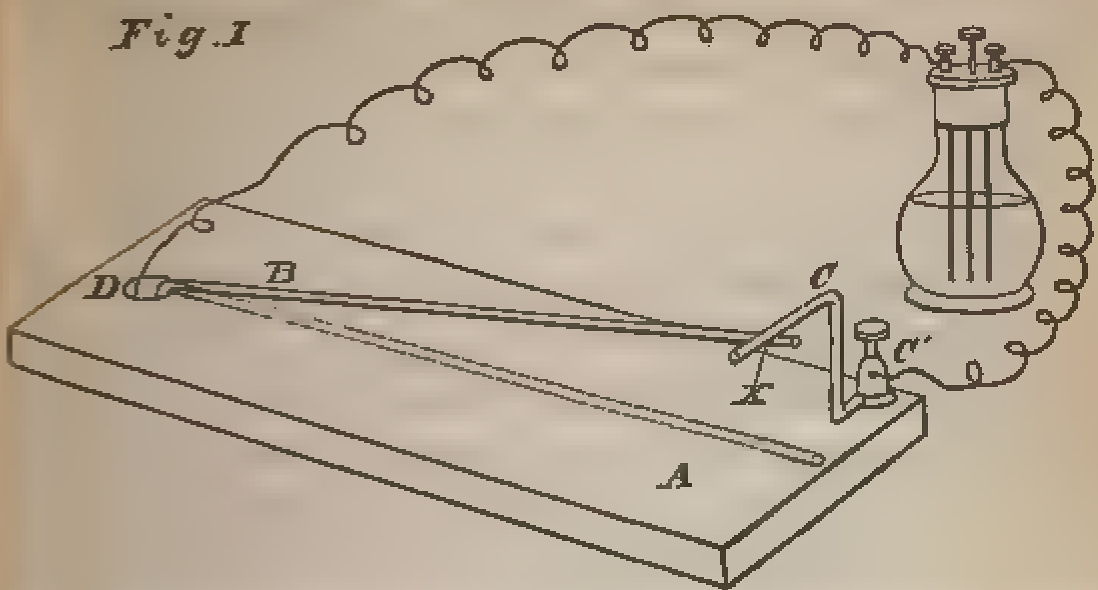
2. If two short wires, which are attached to the two poles of a battery, are brought into contact with one another, especially if they are of steel, a tendency for them to adhere may be felt, similar to the case of two pieces of wax being brought into slight contact and separated again.

3. To demonstrate this fact in a more striking manner, the following arrangement has been devised:—

Fig. I. represents a board of about twelve inches by four, at one end of which is fixed a terminal, and attached thereto is a steel wire C about three mm. thick, bent as shown in the diagram so as to leave a space of two or three inches between wire and board. The *other* end of the board contains a mercury cup D, so that a current may be there made and interrupted without

touching or shaking the board. A steel wire B, about twelve inches long, and bent at one end so as to dip into the mercury cup,

*Fig. 1*




is laid upon the board. One pole of a small bichromate element is connected with the terminal C′, and the other pole with the mercury cup D.

4. If, now, the straight end of the steel wire B is raised until it comes into contact with the bent wire C, it will adhere to it with considerable power; and if the wire B is not *too* heavy (it may be one to three mm. thick), contact at the mercury cup D may now be broken and made any number of times without affecting the adhesion of the two wires B and C.

5. Each time contact is made, a click or snapping sound is heard in the "contact-joint" (as it may be called) X. This click is also heard, but only *faintly*, when the current is interrupted. It may be mentioned that for this experiment it is well to have longer connecting wires, and to make and break contact at a little distance from the board, since the click of the mercury contact may otherwise be mistaken for that of the contact-joint.

6. If the steel wire B is replaced by a stouter one, say about five to six mm. thick, and raised against the bent steel wire C while the battery is connected, it will also be found to adhere, but owing to its greater weight it will subject the contact-joint X to a strain, which is near the breaking-strain; and now, although the current may be interrupted at the mercury cup D without any effect, rupture of the contact-joint, accompanied with a bright spark, will take place at the instant when the circuit is re-established.

7. It is necessary that the battery be connected to the mercury cup and the terminal C′ before the two steel wires B and C are brought into contact, as the effect described will not take place when this order is reversed. It is also necessary that the two steel wires B and C be perfectly clean at the points where they are brought into contact. Rubbing the same with emery paper before the experiment will ensure success.

8. The greatest effect of the adhesion is obtained if two sharp edges are brought into contact at about right angles to each other, while large flat surfaces will only adhere with difficulty or not at all. The adhesion of sharp-edged contacts is considerable, and in the case of hard steel, with a single bichromate element, is so great that it will resist a strain of half a pound and more. By the employment of sharp edges, all metals can be made to adhere easily with more or less tenacity.

9. This fact has led to the construction of a balance, which is represented in Fig. II., for the purpose of ascertaining the degrees of adhesion of different metals.

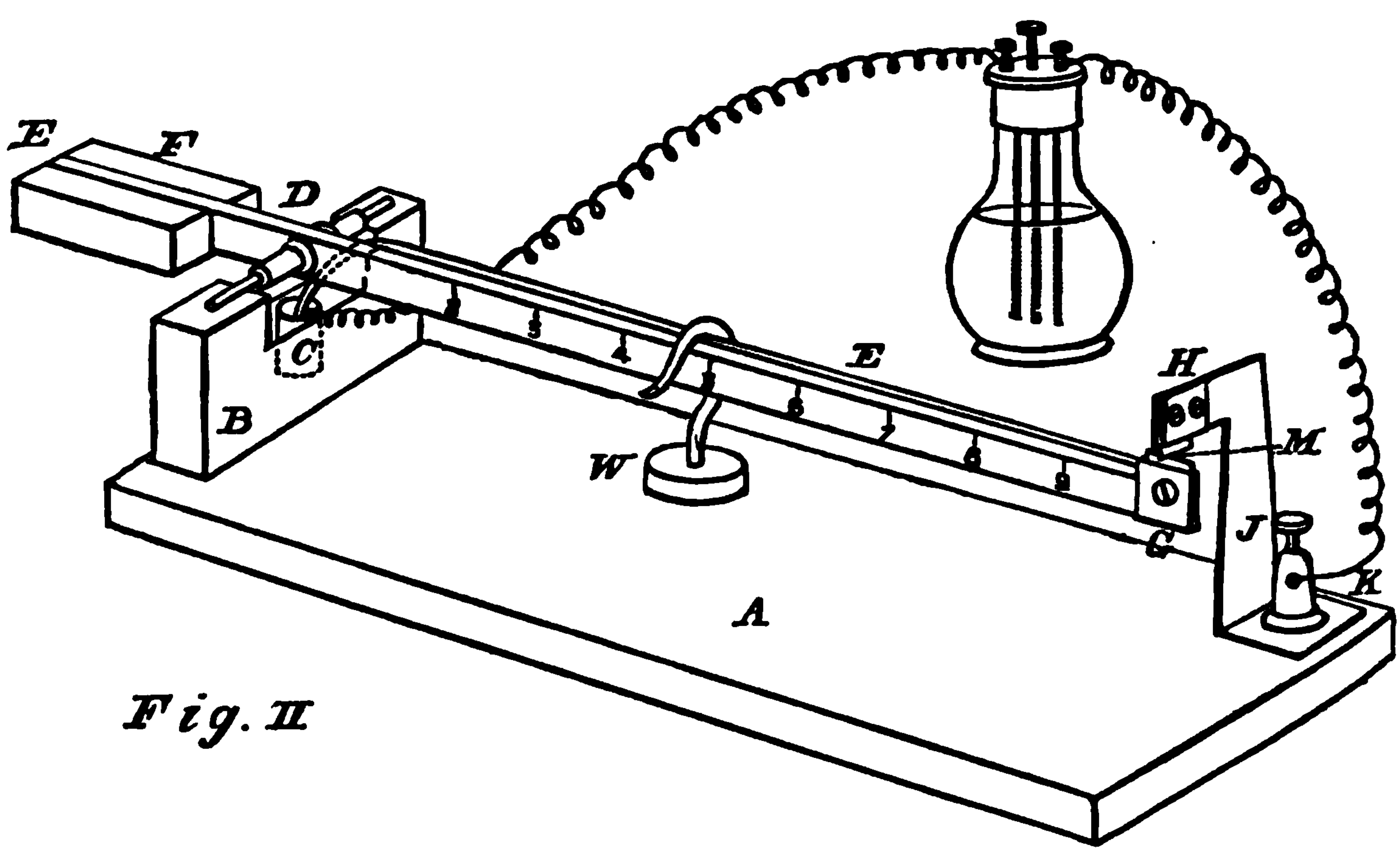



Fig. II

On a board A, about twelve inches long, is a support B, containing a mercury cup C. On the two projections of the support B rests a spindle or axis D, to which is fixed a wooden beam or lever E. This carries a counterbalancing weight F at its short end, and its longer end is furnished with a brass screw-clamp G, to which is attached a wire which runs along the beam E and dips into the mercury cup C. Above the clamp G, and at right

angles to it, is another screw-clamp H on a brass upright J, which is fixed on the board A with a binding screw or terminal K.

The two screw-clamps G and H are for the purpose of holding the metal contact pieces which are under examination, and one of which is represented in Fig. III. on an enlarged scale, the contact

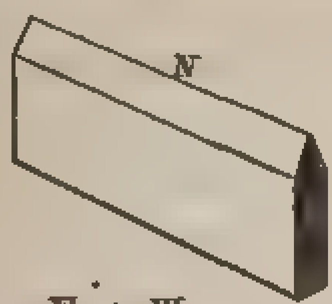



Fig. III

edge N being formed by two bevels at an angle of 45° with each other. A number of pairs of contact pieces of different metals being previously prepared, one pair is fixed in the two clamps, and connections are made from the terminal and the mercury cup to a single bichromate element. The lever or beam E is now raised until the contact pieces meet at M, when they will adhere, and by weights of different value (W), which can be moved along the lever E, the adhesive strength of the contact-joint is ascertained. In this manner the following list has been prepared, about six experiments having been made with each metal, and the average taken in each case. The figures given are the weight in grammes which each contact-joint is capable of supporting:—

| | Grammes. |
|---|---|
| Copper ... ... ... ... ... ... ... | 0·15 |
| Silver ... ... ... ... ... ... ... | 0·15 |
| Aluminium ... ... ... ... ... ... | 2·5 |
| Brass ... ... ... ... ... ... ... | 8·5 |
| Zinc ... ... ... ... ... ... ... | 11· |
| Tin ... ... ... ... ... ... ... | 14· |
| Gold ... ... ... ... ... ... ... | 17· |
| Lead ... ... ... ... ... ... ... | 18· |
| German Silver ... ... ... ... ... | 28· |
| Platinum ... ... ... ... ... ... | 42· |
| Iron ... ... ... ... ... ... ... | 85· |
| Steel, soft ... ... ... ... ... ... | 100· |
| Steel, hard ... ... ... ... ... ... | 225· |

10. It may be worth mentioning that after the contact-joint M is made, it is immaterial whether the battery is disconnected or not; it does not appear to affect the degree of adhesion, nor is the effect in any way dependent on the direction of the current.

11. The values given in the above list are in each case for a *pair* of contacts of the *same* metal. When, however, contact is made between two different metals, it appears that the adhesion obtained is reduced to that due to the metal which stands lowest in adhesive power on the above scale; thus, a contact between copper and steel would only exhibit the same amount of adhesion as one made between copper and copper. A sufficient number of experiments have, however, not yet been made to fully *establish* this fact.

12. Examination of a contact-joint under the microscope leaves no doubt that actual fusion or welding is the cause of adhesion. If it were possible to make a contact so suddenly that ample conducting power for the current were provided without loss of time, no such heating effect could take place, and therefore there could be no adhesion. But when two sharp edges are approaching one another, the first touch of contact must be exceedingly small in extent, and therefore of comparatively large resistance.* Heat at the points of contact must be the result; and if metals were non-conductors of heat while they retained their conducting power for electricity, the current would heat the immediate points of contact *only*, and they would probably remain heated as long as the current was passing. Metals being, however, *good* conductors of heat, it is reasonable to suppose that the welding heat produced by the resistance at the beginning of contact reaches considerably

* Extract from Faraday's Researches, 1,074:—"A voltaic spark is generally due to the ignition and even combustion of a minute portion of a good conductor; and that is especially the case when the electro-motor consists of but one or few pairs of plates. This can very well be observed if either or both of the metallic surfaces intended to touch be solid or pointed. The moment they come in contact the current passes; it heats, ignites, and even burns the touching points, and the appearance is as if the spark passed on making contact, whereas it is only a case of ignition by the current contact being previously made, and is perfectly analogous to the ignition of a fine platinum wire connecting the extremities of a voltaic battery."

beyond the points actually in contact. In fact, in the case of bright steel—which may be called a self-registering thermometer, through the different colours produced by oxidization, and which are left on the surface after heating—it may be seen, by the aid of the microscope, that such is the case.

13. It will follow therefore, as a secondary effect, that the surface of contact becomes larger in extent during the state of incandescence when also the welding together of the points in contact takes place. The resistance is thereby much reduced, and the cooling of the now welded contact must follow. A certain small degree of resistance, and consequently a corresponding degree of heat, must, however, remain during the continuation of the current. But when the circuit is now broken, the contact-joint will acquire the temperature of the surrounding parts. The contraction which must accompany this effect will account for the faint click which is heard at the time.

14. At the moment, however, when the circuit is re-established, a louder click is heard at the joint; or, if the same be under strain, the joint itself may be ruptured altogether. The *former* of these effects must be owing to the sudden expansion of the contact-joint by the heating effect of the current, while the *latter* is probably due to the impulse given by the expansion of the joint to the weight or mass which causes the strain.

15. If a thin steel wire B, which does not exert much strain upon the contact-joint (it may be one to two mm. thick), is used with the instrument represented in Fig. I., the following experiment may be made:—

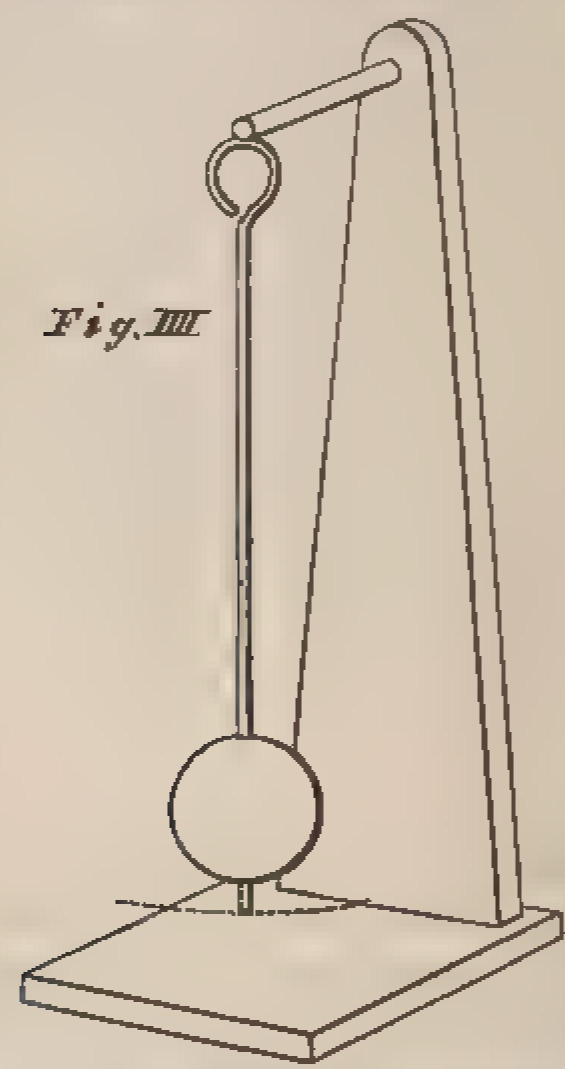
Let *one* bichromate cell be connected, and the contact-joint made by raising the wire B against C. The circuit may now be broken and made at the mercury cup, as before described, and the wire B will remain suspended. But if a current from *two* cells be now allowed to pass through the joint, the wire B will fall on completing the circuit. A similar effect will take place if a current from *three* cells is allowed to pass through a contact-joint which was made with only *two* cells. It appears therefrom that on passing a current, *stronger* than that with which the contact-joint was made, heat is again produced by the resistance

of the joint. It thereby loses its adhesion, and the wire B must fall. It also appears that the current provides for itself a passage across the contacts in proportion to its strength.

16. Another effect worth mentioning—which takes place, however, in the case of *steel only*—is the *hardening of the contact-joint.* The rapid cooling of the points of contact, after having been intensely heated, produces an effect similar to that of immersing red-hot steel into water. A test with a fine file on a soft steel contact-piece, where several contacts have been made side by side, will prove the above statement, the points of contact proving so hard that they will scratch the file.

17. A pendulum suspended by contact, as shown in Fig. IIII.,

Fig. IIII



will oscillate for some time, and a bar suspended as shown in Fig. V. will even prove the joint to resist a small amount of torsion. This shows that a certain amount of elasticity exists at the contact-joint.

18. The occasional sticking of contacts in relays may be accounted for in the following manner :—

*As long* as the two surfaces which make the contact remain

at, so that ample passage for the current is obtained at the ment of completing the circuit, little or no heating effect will ke place.

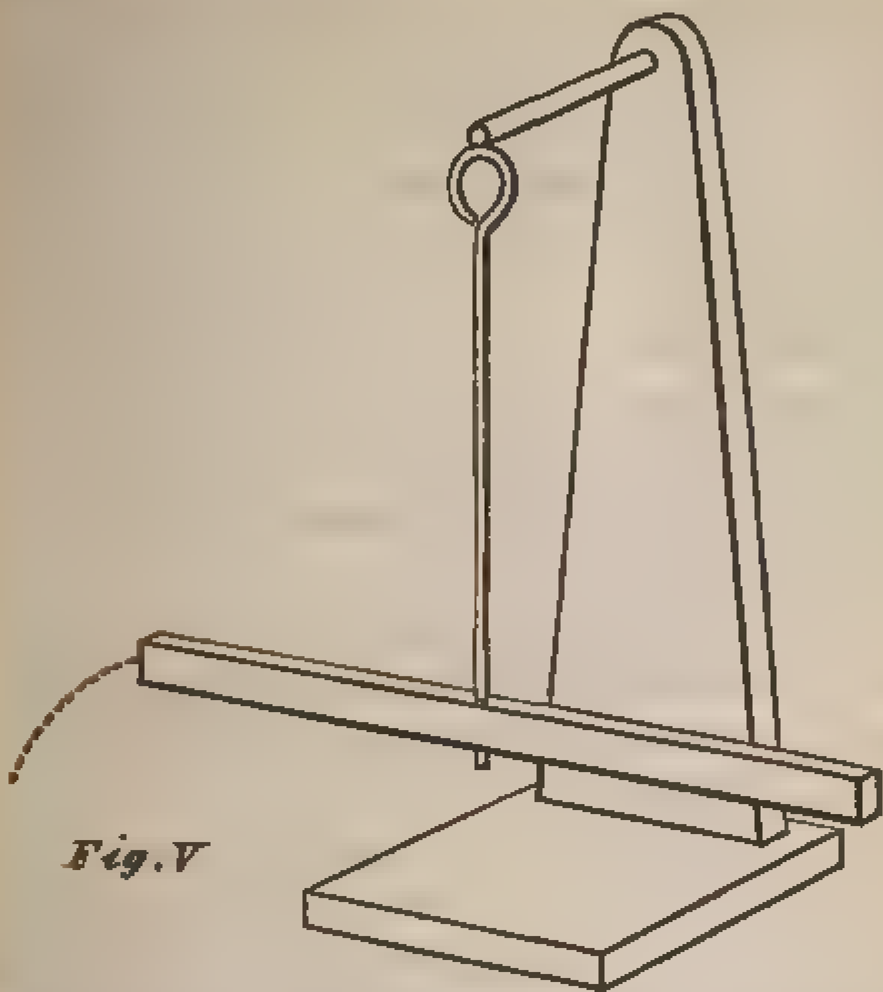


*Fig. V*

When contact is *broken*, however, a spark is always the nsequence. By the repetition of these sparks, the contact-rfaces are burnt and become uneven.

The making of contact thereby becomes more and more perfect and of greater resistance, until some raised point *only* akes contact; the heat then produced causes the melting and dhesion of the contact.

It seems, then, that the primary cause of the sticking of relay ntacts is to be looked for in the effects of the "breaking spark," d therefore any device which will prevent or lessen this effect ill also prevent the adhesion of relay contacts.

(Mr. Stroh then explained the apparatus, and repeated the fferent experiments described in the paper, and supplemented e same by the following remarks).

A glance at the list of metals before us, down as far as atinum, shows that the best conductors for electricity, such as

copper and silver, possess the smallest degree of adhesion; while the worse conductors, such as German silver and platinum, adhere more freely. It would appear that it is owing to the greater resistance, and consequently the greater degree of heat produced by contacts of bad conductors, that the degree of adhesion of the last-named metals is so far in advance of the first. Hardness of the metals, however, as well as their tensile strength, fusibility, and other qualities, play probably a part in forming the various degrees of adhesion.

It is very remarkable to notice how very far iron and steel are above the other metals as regards their power of adhesion. This may possibly be accounted for by the fact that steel and iron can be welded easily under ordinary circumstances, while the other metals do not possess this quality.

I cannot, however, explain the great difference between steel annealed and steel hardened; and having shown the experiments, perhaps some gentlemen present may give a clue to the mystery.

The PRESIDENT: We have all been very much interested both in Mr Stroh's paper and in the experiments he has carried out before us. There are two very remarkable facts brought out by these experiments. The first is that, contrary to what one would expect, when these joints are made with steel the effect is to harden the steel. Now we generally assume that when heat is applied to steel it has the reverse effect. But we know that when a current is passed heat is produced, and it does seem that the effect of such heat on the steel joint is to produce hardening, and, as Mr. Stroh says, to a considerable effect; and the fact is well worth Mr. Anderson's attention, because I must confess that at the present moment there is something about the matter which is extremely difficult to understand.

The second interesting point is the "click" which is heard whenever a current passes through the joint. It is most distinct. It is another point very difficult to understand, although there is no doubt that it is simply due to the expansion produced by heat at the point of contact, which must have a certain amount of resistance. It also enables us to understand what has been rather difficult of comprehension, and that is the fact that the microphone

is reversible. Professor Hughes has shown by experiments that in the microphone there is a bad contact which offers a considerable amount of resistance, and owing to the pressure produced by the sonorous vibrations this contact is made better or worse, and curiously enough the reverse effect is experienced in the case before us. So that the rapid alternation of the contacts of the microphone produce the tones given out; for we all know that the tones of our voices and notes of music are simply due to the fact that clicks or motions are reproduced so frequently that at last they bring notes to our brain.

Professor AYRTON: As a proof that a break in the circuit is not necessary to enable a microphone to act as a receiver, I may mention that last week I was shown by the Count du Moncel a microphone receiver in which there was no break whatever. It was simply a piece of iron wire through which the currents sent by the distant microphone were transmitted, and that continuous piece of wire was quite sufficient to emit sound when intermittent currents were sent through it, no magnets or coils of wire of any kind being employed. The sounds emitted by such a receiving microphone, or whatever this simple straight bit of iron wire may be called, were sufficiently loud to enable a conversation transmitted by a microphone at the distant end to be heard.

The hardening of the steel in Mr. Stroh's experiments is no doubt due to the sudden cooling of the heated contact in consequence of the large mass of cold metal rigidly attached to it.

A ballot having taken place, at which the following gentlemen were elected

*As Associates:*

| | |
|---|---|
| J. Berly. | Somerset R. French. |
| William Carson, M.I.C.E. | A. Graves. |
| A. C. Cockburn. | Philip Harbord. |
| Alfred Coleman. | Edward F. Heath. |
| M. Cooper. | George J. Heraghty. |
| R. E. Crompton. | Francis M. Rogers. |
| Harold Dowson. | Lionel Shorrock. |
| Henry Ferguson. | Arthur H. Vesey. |

the Meeting adjourned until Wednesday, April 28th.

## INDUCTION FROM WIRE TO WIRE IN TELEGRAPH LINES.

Mr. Spagnoletti, considering that his remarks, as they appeared on page 79, No. 31, Vol. IX. of the *Journal*, did not express his meaning completely, desires to substitute the following :—

"About the middle of 1870 reports of 'contact' on Post Office wires were made, but as I could not find any by testing, and as after several efforts the complaints continued, I made further enquiries into the cause. The poles not being earth-wired, I thought might probably be a cause, and I had arms and poles fitted carefully with earth wires. Still, complaints of 'contact' were made. I then went to the City office to see it, and gave it as my confident opinion that it was not 'contact' but 'induction,' due to the strong power used for the high-speed instruments (Wheatstone's automatic), the use of which was then being extended rapidly, and it was on my persisting that it was 'induction,' that when wires were disengaged on the London and North Western Railway, through repairs being done to the Dublin cable, that experiments were made on these wires by the Post Office authorities, the result of which experiments confirmed my opinion that it was 'induction.' This was the first case of the inconvenience of 'induction' being felt on over-head or open wires; hence the doubt held until my opinion had been confirmed.

"After we had had all the poles and arms earth-wired, as the complaints of 'contact' still continued, I made a special test of many of these earth-wires on arms and poles, but they all proved good; but on reaching Birmingham I found a peculiar circumstance—I could not get a deflection on the galvanometer. All connections and the battery were carefully examined, and as all appeared right we tried again, but with the same result. I then had galvanometer placed in circuit without the battery, and found we obtained a deflection by earth current of between 50° and 60°, which was strong enough to establish equilibrium when battery was used, hence no deflective. We tried the earth first on the rail, but thinking that the zone of the galvanised wire might act

with the iron rail and form a battery, I tried it with the earth direct, and the result was just the same—it seemed a local earth current.

"The apparatus we used was rigged up by Inspector Miller. An ordinary vertical galvanometer and four-cell ordinary Daniell's battery, placed on the top of a thick stick with a copper funnel-shaped ferrule with a sharp point to drive into the earth, and a wire running up the stick connecting the ferrule with one pole of the battery for earth contact, it is possible that the copper ferrule and zinc galvanising of earth wire on the pole formed the battery, but if this was so, why should it confine itself to Birmingham to act and not elsewhere?

"The line at this portion of the railway is an embankment and made ground: whether there was anything in the composition of the earth forming the embankment I cannot say."

# ABSTRACTS.

## A. TOEPLER—INDUCTION MACHINES.

(*Electrotechnische Zeitschrift, H. II., February*, 1880, *pp.* 56–60.)

Static induction machines (like those of Holtz) and dynamo-electric machines are compared together at considerable length, and it is explained how the Holtz machine can produce a small quantity of electricity at a high potential if the prime conductor be insulated, whereas the continuous currents that can be produced are exceedingly small. The author describes a static induction machine which he has constructed somewhat on the principle of putting a number of galvanic cells in parallel circuit. A horizontal axis carries a collection of little circular glass plates at small distances from one another. In alternate spaces between the revolving glass plates are placed well-insulated inductors, and in the spaces which are not filled there is a system of combs with their pointed ends turned in the direction of rotation. The two end glass plates are used as in the Holtz machine to increase the charge in the inductors while the remainder are employed to cause an electric current to flow into the combs and out through the wire attached to them. Using twenty glass plates, thirteen centimetres in radius, the whole, independently of the rotating mechanism, only occupying 0·05 cubic metres, and making twenty rotations per second, a current strong enough to work a Siemens' polarised relay, and twenty times as strong as is obtainable with a Holtz's machine, was produced. The best action of the machine was obtained by employing it to charge a battery of Leyden jars which were being constantly discharged by sparking. Charging eighteen great Leyden jars for 0 6 second, a spark was obtained which could heat five platinum wire. To work the machine continuously required the expenditure of four metre-kilogrammes of work per second, and the efficiency of the arrangement was about one-half. As this amount of work is less than one man can furnish, it is desirable to increase the number of glass plates and turn at a higher speed. Charging and discharging a large Leyden jar forty times a second, the discharge spark being one centimetre long, the noise was insufferable, the single sparks were indistinguishable, and a light of one to four candle-power was obtained. Three such induction machines being coupled together, and the battery of Leyden jars being discharged eight to ten times a second through a platinum wire 0·2 mm. in diameter, the wire remained permanently hot. The current could decompose water, but it required thirty-eight minutes to produce one cubic centimetre of explosive gas. The insulating cement employed in the construction of the induction machine deteriorates from use so that it has to be regularly renewed.

## KASSMANN—ON THE INFLUENCE OF ATMOSPHERIC ELECTRICITY ON LONG UNDERGROUND TELEGRAPH CONDUCTORS.

(*Electrotechnische Zeitschrift, H. III., March*, 1880, *pp.* 93, 94.)

There is no doubt about the fact that atmospheric electricity affects subterranean lines, and this occurs through the discharge of thunderclouds or similar causes producing currents in the earth over the wires, and also because the underground wires, having the same earth connections at the end as certain overhead wires coming to the same offices, are affected by the disturbances in these wires. The German telegraph administration made last summer a series of enquiries as to how far these disturbances affect the work of the great underground conductors of the empire, and they have found that in the underground system the disturbance is very much less than in the overhead system. Again, they found that on the underground system disturbances showed themselves to be stronger in the Hughes system than in the Morse. In the Hughes, false signals occurred, single letters and even whole words being omitted, whereas in the Morse only a few signals were slipped. The disturbances were especially great when thunderstorms burst over the places where the lines went to earth; but, indeed, the disturbance was always great immediately underneath discharging thunderclouds, even when there was no earth connection, and they seemed to be as great whether the thunderstorm crossed the line or went along the line. Lastly, when disturbances occurred near one end of the wire going to earth, very little effect was seen at the other. During a very severe thunderstorm which showed itself at one end of the line, and produced frequent and strong discharges, the whole apparatus attached to the overground system was so much affected that messages could not be sent, butal though the thunderstorm was moving in the direction of the underground wires so little disturbance was observed on them that the work went on in the usual way. Suddenly, however, a Morse instrument attached to one of the underground wires stopped working, and was found to be damaged, and a horse in a building about five metres from the underground wire was also hurt by the shock. But it was found that this underground wire was connected with earth by the same copper wire which served for several overhead lines, and on one of these overhead wires a galvanoscope was simultaneously injured. The injury on the instrument attached to the underground wire really arose from the same wire being used as an earth lead for both the underground and the overhead systems.

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## HERMANN ARON — IS THERE DANGER IN CARRYING A LIGHTNING CONDUCTOR INTO A POWDER MAGAZINE?

(*Electrotechnische Zeitschrift, H. III., March*, 1880, *pp.* 94–97.)

If any conductor be exposed to the action of the clouds one or other of two things has to be feared, either a direct discharge from the clouds into the conductor, or a "back stroke," that is, a discharge from the conductor into the earth on the sudden neutralisation of the electrified cloud which has induced the charge in the conductor. If a body be enclosed in a case formed of a

*perfectly* conducting substance, it is, as is well known, screened from both these actions; but if the screen has only limited conductivity, like the earth's crust, effects in an underground wire may be induced by atmospheric causes. To test this an ordinary electric machine was employed, and as a detector of current either a frog's leg or a telephone. One end of a coil of insulated copper wire 122 metres long, was successively attached to either the frog's leg or to one end of the telephone, another wire passing from the frog's leg or from the other end of the telephone to earth. In a distant room, in which the greater part of the wire was coiled up, the other end was insulated, and sparks were drawn from a neighbouring electrical machine by a conductor attached to the earth. Each time a spark passed, the induced effect was visible on the distant frog's leg or telephone, showing that the apparatus was sensitive for detecting back stroke in overhead wires. The experiment was then varied by placing the coil in a tub of salt water, under the water but near the surface, and connecting the water with the earth. It was then found that no effect could be observed either in the distant frog's leg or in the telephone on causing sparks to pass from the machine into the water, provided that the water made a good earth. When, however, twenty Siemens units were inserted between the water and the earth faint sounds were heard on the telephone, and on inserting eighty Siemens units visible motion was observed in the frog's leg. Experiments were then made to prove that the portion of the covered wire immersed in the water was well insulated, so that the effect observed could not be due to leakage of electricity from the water into the wire, but was really an inductive action in the wire from which the surrounding water did not shield it. Dr. Siemens mentioned to the author of the paper that in the same way sparks were to be feared near the ends of underground wires well insulated from the earth and buried in dry ground.

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## UNDERGROUND TELEGRAPH WIRES.

(*Electrotechnische Zeitschrift, H. III., March,* 1880, *pp.* 107, 108.)

The underground telegraph lines established by the German postal and telegraph administration in 1876–79 have a length of 2,274 miles, consisting of 17,380 miles of wire. The article gives the principal towns connected in this way with the distances between them, and states the names of other numerous lines which are to be finished in 1880. The test thus given to the system has led to such satisfactory results that it is being introduced into other states, the most important being France, where a credit of eight million francs has been given for this purpose for a length of 600 miles.

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## A. J. FROST—MEMOIR OF SIR FRANCIS RONALDS, F.R.S.

(*The Ronalds Catalogue, published by the Soc. of Tel. Eng., pp.* 7-27.)

Francis Ronalds was born in London on the 21st February, 1788, and was educated at a private school. At an early age he showed a taste for original experiment, and constructed electrical and scientific apparatus of various kinds. He was also an excellent draughtsman.

In 1814–15 he contributed several papers to the *Philosophical Magazine*, and having at this time become acquainted with M. De Luc, who was then engaged in a series of interesting electrical experiments, he was induced to give his attention to the practical development of science.

It is to the invention of an electric telegraph in 1816 that the name of Ronalds will be best remembered, although the writer of the Memoir has shown that in other respects he contributed materially to the advancement of science.

The telegraph of Ronalds is briefly described in the Memoir, together with some extracts from his work published in 1823, entitled "Description of an Electrical Telegraph, and of some other Electrical Apparatus." This telegraph was erected at Hammersmith, and consisted of eight miles of overhead and 525 feet of underground line, the latter being the line he proposed to adopt. This consisted of copper wire enclosed in glass tubes, the joints in the tubes being made with wax. The tubes were then laid in a wooden trough lined with pitch and covered with wood, and the whole buried in the earth.

The method of conveying intelligence which he adopted may be briefly described as follows:—At each end of the line was placed a clock beating dead seconds, having upon its seconds arbor a light circular brass plate divided into twenty equal parts. Each division was marked with the letters of the alphabet leaving out the letters J, Q, U, W, X, and Z; there were also a series of figures from 1 to 10, and ten preparatory signs. Before or over this disc was fixed another brass plate, which had an aperture of such dimensions that whilst the disc was carried round by motion of the clock only one of the letters, figures, or preparatory signs upon it, could be seen through the aperture at the same time. In front of the clocks a Canton's Electrometer of pith balls was suspended by an insulated wire communicating with a cylindrical electrical machine of six inches diameter, which was connected with the line. These clocks were adjusted (by a method described) to go synchronously.

After a large number of experiments had been made with this telegraph and its practicability for conveying messages had been thoroughly proved, Ronalds decided upon bringing his invention to the notice of the Government. This he did in a letter to the Admiralty, dated the 11th July, 1816, after which he received the now historical note from the Secretary to the Admiralty, dated the 29th July, 1816, saying that telegraphs of any kind were wholly unnecessary. The whole of this correspondence is given in the Memoir.

The semaphore telegraph, which was then in use, is referred to. This telegraph remained in use for at least 27 years after the date quoted, and formed the subject of a Parliamentary return, dated the 2nd May, 1843, in which it is stated that during three years it was found impossible to use the telegraph from the Admiralty on no less than 323 days.

Besides referring to the many uses to which the telegraph was applicable, he foretold in his work the "retardation of current," a subject which occupied the attention of electricians during the early days of submarine telegraphy. He also provided for repairing and testing the line, and says, "Let us have *electrical conversazione* offices, communicating with each other all over the Kingdom."

The telegraph of Ronalds was worked by frictional electricity, and although in its crude form it might not have been at once capable of doing all that he promised, yet had the Admiralty given the invention a fair trial in 1816, it might have been easily improved, and the whole history of the telegraph would in all probability have been changed; at any rate its powers would have been exercised, and the world might have profited by its advantages long before 1837.

In the work referred to he described several novel electrical instruments.

Reference is made to the controversy between Cooke and Wheatstone, and several interesting letters from Cooke and others are quoted relating to the electric telegraph.

It is also stated that Wheatstone when a boy was present at some of the early experiments at Hammersmith.

In 1825 Ronalds invented a perspective tracing instrument to facilitate drawing from nature, a description of which he published. He also published, with Dr. Blair, in 1836 "Sketches at Carnac: or notes concerning the present state of the Celtic antiquities in that and some of the adjoining Communes." The tracing instrument was used in procuring exact perspective projections from given noted stations of the antiquities described and illustrated in the work.

In 1843 Ronalds was appointed the first Hon Director and Superintendent of Kew Observatory—a position which he held for nine years. During this period he wrote several full and interesting reports to the British Association, of which body he was for many years a Member of Council.

He was on the 1st February, 1844, elected F.R.S.

During his directorship of Kew Observatory he invented many important instruments for observing meteorological phenomena, the principal and most important being his application of photography to the registration of atmospheric electricity, and terrestrial magnetism, &c. These inventions contributed in no small degree to the advancement of meteorological and magnetical observation, and they are described and illustrated in the *Philosophical Transactions* for 1847, Part I.

Ronalds received a grant of £50 from the Royal Society to assist him in the construction of his self-registering instruments, and he received a further grant of £100 for the purpose of an experimental trial of them. At his death he bequeathed a sum of £500 to the Royal Society in recognition of the grants they had made to him.

In 1852, on his retirement from the direction of Kew Observatory, he received a small Civil List pension of £75 per annum for "his eminent discoveries in electricity and magnetism"

The remainder of his life was devoted to the compilation of his Catalogue of electrical works, and to the formation of the Ronalds' Library, which is now in the possession of the Society.

On the 18th February, 1870, he received the honour of knighthood for, as is stated in a letter from the Right Hon. W. E. Gladstone, "his early and remarkable labours in telegraphic investigations"

*Sir Francis* Ronalds died on the 8th August, 1873, and was buried at Battle.

## W. GIESSE.—REMANENT CHARGE IN LEYDEN JARS.

(*Annalen der Physik und Chemie*, B. IX., H. 2., 1880, pp. 161 208 )

Kohlrausch published in *Poggendorff's Annalen*, in 1854, the first quantitative experiments on this subject, and gave an empirical formula which was sufficiently accurate for use in his experiments with Weber when making the first determination of the ratio of electro-static and electro-magnetic units. Kohlrausch did not base his formula upon any theory of what occurs in the insulator. The author comments upon the methods which have been adopted to explain the phenomenon in accordance with the received views on electricity; shows that Wüllner's formula (which contains an exponential of time) is unsatisfactory, and refers to Bezold in *Pogg. Ann.* 1861. Maxwell's theory of heterogeneity of the dielectric he says is, like other theories, too artificial; and Maxwell himself lays particular stress on the possibility of other yet unknown phenomena being the cause of remanent formation. Riemann, proceeding on the supposition of an anti-electric power in matter, obtained in 1854 a differential equation, which he has since made use of; but he compared his formulæ with the results of experiments of an unsuitable kind. The author shows how unsuitable were observations on the change of potential in an insulated jar, and he proceeds to describe his own experiments to measure the current at every instant flowing into a jar under the influence of a constant very large battery. This current could not be measured directly by a galvanometer, and he obtained a measure by suddenly insulating, measuring change of potential in one-quarter or one minute or longer, then connecting again with battery and repeating his short insulation as soon as the needle of his electrometer became steady. The author writes at length upon difficulties of observation and upon the details of his method.

He first tried one hundred Leclanché's cells, but the electromotive force was too variable, and failing to get satisfactory results with Lodge's form of cell he used a much more constant Daniell's battery of his own construction. In measuring time he might possibly have met with errors of 2 per cent. in insulations of one-quarter minute, measurements of longer periods having smaller errors. In considering his method of measurement of current he first examines an expression for potential at the end of a given time of insulation, a single exponential of the time, and shows that this is not sufficient for his purpose. When P, the electromotive force, is constant the current is

$$P\,(a + b\,Q_t)$$

where Q vanishes for negative values of $t$ and for $t = \infty$. He finds that $Q_t = t^{-m}$ is an expression which satisfies his experiments very well indeed, but it is open to the objection that when we construct the corresponding formula for current under varying electromotive force on the law of superposition, the expression is certainly wrong. Hence the author says that his empirica formula must only be used to correct his observations, and he devotes several pages of his paper to the explanation of these reductions. He then proceeds to give tables showing the correctness of the empirical formula

$$i = a + b\,t^{-m}$$

$i$ being current and $t$ time from the beginning of charging, and afterwards shows that the temperature of a Leyden jar has a very great influence upon the values of the constants $a$, $b$, $m$, so that it is absolutely necessary during an experiment to keep a condenser at constant temperature. The following values of these constants have been selected from the paper:—

| | $a$. | $b$. | $m$. | Temp. |
|---|---|---|---|---|
| Flask No. 1. Glass of Warmbrunn. | ·001095 | ·016955 | 0·6665 | ? |
| | ·00131 | ·02058 | 0·6282 | 21·65 |
| | ·00083 | ·021212 | 0·6193 | 21·4 |
| | ·000749 | ·017844 | 0·6710 | 14·6 |
| Flask No. 2. Very non-conducting French Crystal Glass. | —·000023 | ·005531 | ·7031 | 20·55 |
| | ·00016 | ·005732 | ·7684 | 19·9 |
| | ·000105 | ·005985 | ·7406 | 23·0 |
| Flask No. 3. | ·00103 | ·029704 | ·6667 | 18·05 |

The author also gives results obtained by charging condensers from intermittent current dynamo-electric machines.

The rest of the paper is devoted to a long and careful process of testing the truth of Riemann's hypothesis; and the author shows that whether we assume the original hypothesis of Riemann, or its form as modified by Helmholtz (in which the electricity is supposed to enter the dielectric only to a short distance), we find that it disagrees with his observations. Indeed, for five values of the observations, the deviations from truth are greater than the empirical formula gave for all the twenty observations, and there seem to be radical discrepancies.

The outsides of his condensers were of tinfoil, the insides concentrated sulphuric acid, the author found that he had to keep them short-circuited for weeks to get rid of remanent electricity, and that there were independent charges in the jar, which did not, however, amount to more than 0·3 of a Daniell. He discusses this part of his subject at some length.

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|---|---|---|

The Eighty-eighth Ordinary General Meeting of the Society was held on Wednesday evening, April 28th, 1880, at the Institution of Civil Engineers, 25, Great George Street, Westminster—Mr. W. H. Preece, President, in the Chair.

The minutes of the last meeting were read, and the names of transferees and new candidates announced, after which the President asked Professor Ayrton to read a

## NOTE ON THE BURNING OF THE POSITIVE CARBONS IN THE ELECTRIC ARC.

By J. D. F. Andrews (*Student*).

I have had opportunities of making some measurements of the craters of positive carbons with various strengths of current, at Messrs. Siemens Brothers' works, and I find that the area of the crater is directly proportional to the quantity of current producing it. To make measurements in this way, the light should burn quite steadily and quietly for at least half-an-hour with an arc of about $\frac{1}{4}''$; with this arc the depth of the crater is very small, and need not be taken into consideration. When the crater is cold, the diameter may be measured with a rule divided into 64ths or 100ths of an inch. The table shows how measurements taken in this way agree with those taken by other methods; the 42 weber measurements is the mean of 30, and may be taken as a constant.

| Diameters of Craters. | Areas of Craters. | Quantities of Cur. in webers, measured. | Quantities of Cur. in webers, cal. from Craters. |
|---|---|---|---|
| ·140″ | ·0196″ | 9 | 9·9 |
| ·156″ | ·0243″ | 12 | 12·4 |
| ·186″ | ·0272″ | ... | 13·8 |
| ·203″ | ·0324″ | ... | 16·5 |
| ·266″ | ·0556″ | 29 | 28·3 |
| ·326″ | ·0825″ | 42 | 42·0 |
| ·453″ | ·1602″ | 81 | 81·6 |

If the light should be accompanied with noise, it will be found that the burning end of the positive carbon is covered with a number of small craters—showing that the arc had been moving about irregularly; and on watching the arc it will be noticed, besides its moving about, that a number of very small arcs are trying to spread over the end of the positive carbon, each detonating the air, and thus causing the hissing noise, which I venture to say is not due to impurities, but to the positive carbon being too hard to disintegrate to its proper shape, and consequently having too large a surface for the arc to try to spread itself over; if the arc be disturbed by blowing it or shaking the carbons, the noise will be more readily produced.

J. D. F. ANDREWS.

Charlton.

Professor AYRTON then gave an account of the following paper:—

## ON THE RESISTANCE OF GALVANOMETERS.

By OLIVER HEAVISIDE (Associate).

The well-known result that the resistance of a galvanometer coil of given size should equal the external resistance in order to obtain the greatest magnetic force is easily verified. If G is the magnetic force at the centre of the coil for unit current in the coil, and M the magnetic force due to the current $\gamma$, then $M = \gamma G$. Also, by Ohm's law, if R and $r$ are the resistances of the galvanometer and of the rest of the circuit, and E the electromotive force, $\gamma = E \div (R + r)$. Whence

$$M = \frac{E\,G}{R + r}. \quad \dots \quad \dots \quad \dots \quad \dots \quad (1.)$$

Here $G = g\,l$, where $l$ is the length of wire in the coil, and $g$ the mean value of G per unit of length throughout the space occupied by the coil, and therefore the same for different sizes of wire. Now, if we neglect the thickness of the covering of the wire, it is easily seen that the resistance of the coil varies as the square of the length of the wire. Thus, in (1) $G \propto l$ and $R \propto l^2$, and therefore M is a maximum when $R = r$.

In the next place, suppose the thickness of the covering is constant: let the radius of the wire $= y$ and of the covered wire $= y + b$, then the volume V of the coil is

$$V = 4\,l\,(y + b)^2.$$

If $\rho =$ the specific resistance of the wire

$$R = \frac{\rho}{\pi}\,\frac{l}{y^2}.$$

Also $G = g\,l$, as before. Therefore in (1), $G \propto (y + b)^{-2}$, and $R \propto y^{-2}\,(y + b)^{-2}$, and M is a maximum when

$$R : r = y : y + b,$$

or the resistance of the galvanometer should be to the external resistance as the radius of the bare wire is to the radius of the covered wire. (Maxwell, II., Art. 716.)

But if we suppose that the radius of the covered wire bears a constant ratio to the radius of the wire itself, the result is again $R = r$. For let the radius of the covered wire $= \beta\,y$, then

$$R = \frac{\rho}{\pi}\,\frac{l}{y^2}, \qquad V = 4\,l\,\beta^2\,y^2.$$

Thus, in (1), $G \propto y^{-2}$ and $R \propto y^{-4}$, and therefore M is a maximum when $R = r$.

In the above, the form of the channel in which the wire is wound is arbitrary, and the thickness of the wire the same throughout the coil. But when the windings are circles there is a certain form of coil which gives the greatest magnetic force at the centre of the coil for a given length of wire. The wire should be wound in layers on surfaces defined by the polar equation

$$r^2 = x^2 \sin.\,\theta,$$

where $r$ is the distance of a circle of wire from the centre of the coil, $\theta$ the angle between $r$ and the axis of the coil, and $x$ a constant determining the linear dimension of the layer. With this form of coil, if the ratio of the radius of the covered to the radius of the bare wire is constant, the diameter of the wire in any layer should vary as the linear dimension of the layer to get the greatest electro-magnetic effect. (Maxwell, II., Art. 719.)

Under these circumstances, what should the resistance of the coil be? Professors Ayrton and Perry asked this question, and their answer was, $R = r$ again (*Journal Society Telegraph Engineers*, vol. vii., p. 297). For so simple a result to arise out of such complexity is rather striking, and, being lately occupied with a similar question, I looked for the reason of this result. It appears not to depend on the particular form of coil considered, nor on the particular law governing the diameter of the wire in the different layers, but solely upon the assumption of a constant ratio between the radius of the covered and of the bare wire.

Thus, let $y$ = variable radius of the wire itself, and $z$ = variable radius of the covered wire. Then

$$R = \frac{\rho}{\pi}\int\frac{d\,l}{y^2},\ V = \int 4\,z^2\,d\,l,\ G = \int\frac{\sin.\theta}{r^2}\,d\,l = g\,l.$$

But since the coil is a figure of revolution

$$V = 2\,\pi\iint r^2 \sin.\theta\,d\,r\,d\,\theta.$$

Let the limits of integration for $r$ be $x_0 f(\theta)$ and $x_1 f(\theta)$. Then

$$V = 2\,\pi\int_0^{\pi}\{f(\theta)\}^3 \sin.\theta.\ d\,\theta.\ \frac{x_1^3 - x_0^3}{3} = \tfrac{1}{3}\,N\,(x_1^3 - x_0^3)$$

where N is a numerical constant. Therefore

$$d\,V = N\,x^2\,d\,x = 4\,z^2\,d\,l,$$

where $d\,V$ is the volume of the layer corresponding to $d\,x$, and $d\,l$ the length of wire in the layer. Thus,

$$R = \frac{\rho}{\pi}\int\frac{N\,x^2\,d\,x}{4\,y^2\,z^2},\quad G = g\int\frac{N\,x^2\,d\,x}{4\,z^2}$$

and

$$M = Eg\frac{l}{R+r} = Eg\frac{\displaystyle\int\frac{N x^2\,dx}{4z^2}}{\displaystyle\frac{\rho}{\pi}\int\frac{N x^2\,dx}{4y^2 z^2}+r}$$

Let now $z = \beta y$, where $\beta$ is constant, then

$$M = Eg\frac{l}{R+r} = Eg\frac{\displaystyle\frac{1}{\beta^2}\int\frac{N x^2\,dx}{4y^2}}{\displaystyle\frac{1}{\beta^2}\frac{\rho}{\pi}\int\frac{N x^2\,dx}{4y^4}+r}$$

If $y$ is constant with respect to $x$, $l \propto y^{-2}$ and $R \propto y^{-4}$, so that M is a maximum when $R = r$. If $y = a\,x$, the same result follows. But $y$ may be any function of $x$; say $y = a\,\phi(x)$, where $\phi$ determines the law of variation of the radius of the wire from layer to layer, and $a$ fixes the actual size of the wire. Then

$$M = Eg\frac{l}{R+r} = Eg\frac{\displaystyle\frac{1}{a^2\beta^2}\int\frac{N x^2\,dx}{4\phi^2}}{\displaystyle\frac{1}{a^4\beta^2}\frac{\rho}{\pi}\int\frac{N x^2\,dx}{4\phi^4}+r}$$

Let $a$ vary, then, as before, $R = r$ makes M a maximum; for $l \propto a^{-2}$ and $R \propto a^{-4}$.

Thus $R = r$ makes M a maximum when the diameter of the wires in different layers is arbitrary and the form of the layers arbitrary (except that they are similar surfaces of revolution), provided that $z = \beta y$. Other relations between $z$ and $y$ of course give other results.

The following is more general: Take a long wire of circular section, whose radius varies continuously along its length, and let it be covered so that the thickness of the covering along its length varies in the same manner: *i.e.*, $z : y =$ constant everywhere. Now wind this wire into a coil of any shape and section. It will have a certain resistance, and the unit current in it will produce a certain magnetic force at any point. Now, if the radius of the wire is everywhere reduced to $\frac{1}{n}$th part, and the same space is filled, we have everywhere $n^2$ wires instead of one; therefore the magnetic force due to the unit current in an element of length of the original wire is increased $n^2$ times by the unit current now

passing in $n^2$ wires instead of one, and the resistance is increased $n^4$ times by the change. Since the same is true for each element of length of the original wire, it follows that the magnetic force due to the unit current in the whole coil varies inversely as the square of the radius of the wire, and the resistance of the coil inversely as the fourth power of the same. Therefore,

$$M = \frac{E\,G}{R + r} = E\,\frac{A\,a^{-2}}{B\,a^{-4} + r}$$

where A and B are constants depending on the form and dimensions of the coil, and $a$ determines the actual radius of the wire at any part. Vary $a$, then M is a maximum when $R = r$ as before.

A vote of thanks having been cordially voted to Mr. Heaviside, the Secretary read an abstract of the following paper:—

## ON THE DETERMINATION OF THE POSITION OF FAULTS IN A CABLE WHEN TWO EXIST AT THE SAME TIME.

By C. Hockin, M.A. (*Member*).

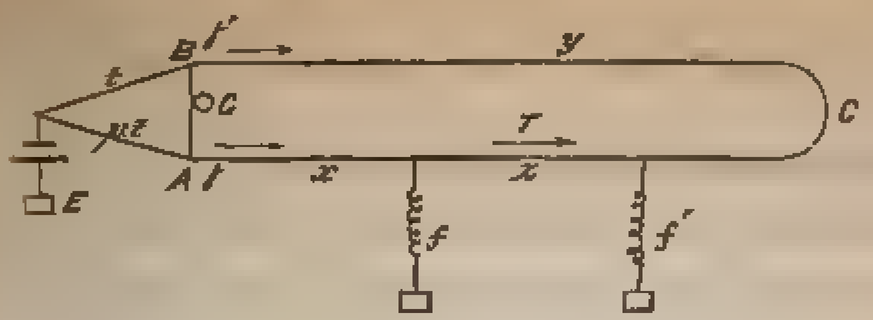
It is well known that the necessary data for determining the positions of two co-existent faults in a submarine cable cannot be obtained by the methods in ordinary use, such as taking the resistance from either end, the conditions at the further end being varied.

As the reason of this is not very apparent, and since a complete analysis of the case has not, to my knowledge, been published, some of your readers may be interested in the following investigation.

Suppose a submarine cable to have two faults in it, and suppose also that a return line, which is perfect, is available.

The data obtainable by measurements of resistance, potential, and current, are as follows:—

1. The resistance from the end A, the end B being free, call this R.
2. The resistance from the end B, the end A being free, call this R′.




3. The resistance from the end A, the end B being earthed, call this $r$.
4. The resistance from the end B, the end A being earthed, call this $r'$.
5. The resistance of the circuit A C B, the ends A and B both free, call this $\rho$.
6. The resistance obtained by looping the ends A and B and then measuring the resistance between the looped circuit to earth, call this $\rho'$.
7. Two resistances, $t$ and $\mu t$, may be inserted and adjusted so that no current flows through G, and so the position of a "resultant fault" may be obtained depending on the ratio $\mu$.
8. We may measure the resistance from A and at the same time the potential at B supposed free. If the potential at A is V, call that at B $n$V.
9. The same observations may be made, reversing the conditions at the ends A and B, let $n$ become $n'$ in this case.
10. We may measure the resistance from A and the current flowing through B to earth at the same instant. If V is the potential at A, let $\frac{1}{s}$ V be the current through B at the same time.
11. As before, the conditions at the ends may be reversed, let $s$ become $s'$ in this case.

Let $f$ be resistance of the fault nearest the end A.
Let $f'$ be resistance of the fault nearest the end B.
Let $x$ be resistance between A and the nearest fault $f$ measured along the cable.
Let $y$ be resistance between B and the nearest fault $f'$ measured along the cable.

Let $z$ be resistance between the faults, measured along the cable.

Let $l$ be resistance of whole line so that $l = x + y + z$.

Of the eleven data above mentioned three only (numbers 5, 6, and 7) require for their determination a return wire.

At first assume that the resistance of the faults is constant under all conditions.

From the eleven observations as many equations can be formed connecting the four unknown quantities, $f, f', x, y$. It is to be shown, what is not at first sight apparent, that these equations yield but one relation between $x, y, z$, and so do not determine the position of the fault.

The equations may at once be written down.

$$\mathrm{R} = x + \frac{f(z+f')}{f+f'+z} \quad \ldots \quad \ldots \quad \ldots \quad 1.$$

$$\mathrm{R}' = y + \frac{f'(z+f)}{f+f'+z} \quad \ldots \quad \ldots \quad \ldots \quad 2.$$

$$r = x + \frac{f\left(z + \frac{yf'}{y+f'}\right)}{f + z + \frac{yf'}{y+f'}} \quad \ldots \quad \ldots \quad \ldots \quad 3.$$

$$r' = y + \frac{f'\left(z + \frac{xf}{x+f}\right)}{f + z + \frac{yf}{y+f}} \quad \ldots \quad \ldots \quad \ldots \quad 4.$$

$$\rho = x + y + \frac{z(f+f')}{z+f+f'} \quad \ldots \quad \ldots \quad \ldots \quad 5.$$

$\rho'$ is thus determined.

Let $\gamma, \gamma'$, and $\Gamma$ be the currents in $x, y, z$ in the directions indicated by the arrows. Then

$$\rho'(\gamma+\gamma') = \gamma x + (\gamma - \Gamma)f$$
$$\gamma' y - \Gamma z - \gamma x = o$$
$$\Gamma z + (\Gamma + \gamma')f' + (\Gamma - \gamma)f = o$$

liminating $\gamma', \gamma, \Gamma$

$$\begin{vmatrix} \rho', & \rho' - x - f, & f \\ y, & -x \quad , & -z \\ f', & -f \quad , & f' + z + f \end{vmatrix} = o,$$

From the second column of the determinant deduct the difference between the first and third, then

$$\begin{vmatrix} \rho', & -x, & f \\ y, & -l, & -z \\ f', & +z, & f'+z+f \end{vmatrix} = o,$$

Or

$$\rho' = \frac{xzf' + lff' + yzf + xy(f+f') + xyz}{l(f+f') + z(x+y)} \qquad 6.$$

$\mu$ is readily found by observing that $\gamma = \mu\gamma'$. Hence

$$(x - \mu y)\gamma + \Gamma z = o$$

$$\Gamma z + (\Gamma + \mu\gamma) f' + (\Gamma - \gamma) f = o$$

$$\therefore (\mu f' - f) z = (x - \mu y)(z + f + f')$$

$$\mu = \frac{f'x + f(x+z) + xz}{yz + fy + f'(y+z)} \quad \ldots \quad \ldots \quad \ldots \quad 7.$$

For $n$ we have at once

$$n = \frac{f'}{f' + z} \times \frac{R - x}{R} \quad \ldots \quad \ldots \quad \ldots \quad 8.$$

So

$$n' = \frac{f}{f+z} \times \frac{R'}{R' - y} \quad \ldots \quad \ldots \quad \ldots \quad 9.$$

For $s$ the expression is a little more complex,

$$\frac{1}{s} = \frac{1}{y} \cdot \frac{f'y}{y+f'} \cdot \frac{r - x}{x} \cdot \frac{1}{z + \dfrac{yf'}{y+f'}} \quad \ldots \quad 10.$$

So

$$\frac{1}{s'} = \frac{1}{x} \cdot \frac{fx}{f+x} \cdot \frac{r' - y}{y} \cdot \frac{1}{z + \dfrac{xf}{x+f}} \quad \ldots \quad 11.$$

$f + f'$ have now to be eliminated between these equations.

(1) + (2) − (5) yields

$$R + R' - \rho = \frac{2ff'}{f + f' + z},$$

(1) − (2) + (5) yields

$$R - R' + \rho - 2x = \frac{2fz}{f + f' + z},$$

So

$$R' - R + \rho - 2y = \frac{2f'z}{f + f' + z}.$$

Also from (5)

$$l - \rho = \frac{z^2}{f + f' + z}$$

we have therefore

$$2 f f' = \frac{z^2}{l - \rho} (R + R' - \rho) \quad \dots \quad \dots \quad 12.$$

$$2 f = \frac{z}{l - \rho} (R - R' + \rho - 2 x) \quad \dots \quad 13.$$

$$2 f' = \frac{z}{l - \rho} (R' - R + \rho - 2 y) \quad \dots \quad 14.$$

Comparing the first of these equations with the product of the second and third

$$\left.\begin{aligned} &2 (l - \rho) (R + R' - \rho) = (R - R' + \rho - 2 x) \\ &\qquad (R' - R + \rho - 2 y) \\ \text{or}& \\ &\rho^2 + (R - R')^2 + 2 (R + R') (l - \rho) + 2 (R - R') \\ &\qquad (y - x) - 2 \rho z + 4 x y = o \end{aligned}\right\} 15.$$

the only relation to be obtained between $x$ and $y$.

Equation 3 becomes

$$r = x + \frac{f y z + f f' (y + z)}{y (z + f + f') + f' (f + z)}$$

$$= x + \frac{y (R - R' + \rho - 2 x) + (R + R' - \rho) (y + z)}{2 y + R + R' - \rho + R' - R + \rho - 2 y}$$

$$= x + \frac{y (R - R' + \rho - 2 x) + (R + R' - \rho) (y + z)}{2 R'}$$

Or

$$\begin{aligned} 2 r R' &= R (2 y + z) + R' (2 x + z) - \rho z - 2 x y \\ &= l (R + R') + (R - R') (y - x) - \rho z - 2 x y \end{aligned}$$

$\therefore$ from equation 15

$$\begin{aligned} 4 r R' &= 2 \rho (R + R') - \rho^2 - (R - R')^2 \\ &= 4 R R' - (R + R' - \rho)^2 \quad \dots \quad \dots \quad 16. \end{aligned}$$

From symmetry we have also

$$4 r' R = 4 R R' - (R + R' - \rho)^2 \quad \dots \quad \dots \quad 17.$$

The equation 6 may be written

$$\rho' = \frac{xy\,(f + f' + z) + l f f' + y z f + x z f'}{l\,(f + f' + z) - z^2}$$

$$= \frac{2xy + l(R + R' - \rho) + (R - R')\,(y - x) + \rho\,(x + y) - 4xy}{2l - 2l + 2\rho}$$

$$\therefore 2\rho\rho' = l(R + R') + (R - R')\,(y - x) - \rho z - 2xy \qquad 18.$$

$\therefore$, by equation (15),

$$\because 4\rho\rho' = 4RR' - (R + R' - \rho)^2.$$

Again for $\mu$ we have

$$\mu = \frac{x\,(f + f' + z) + fz}{y\,(f + f' + z) + f'z}$$

$$= \frac{2x + R - R' + \rho - 2x}{2y + R' - R + \rho - 2y}$$

$$= \frac{R - R' + \rho}{R' - R + \rho}$$

$$\therefore \frac{\mu - 1}{\mu + 1} = \frac{R - R'}{\rho} \quad \ldots\ \ldots\ \ldots\ \ldots\ \ldots \quad 19.$$

Also

$$n = \frac{f'}{f' + z} \cdot \frac{R - x}{R}.$$

From equation (1)

$$R - x = \frac{f\,(f' + z)}{f + f' + z}$$

$$\therefore n = \frac{ff'}{R\,(f + f' + z)}$$

$$2n.R = R + R' - \rho \ldots\ \ldots\ \ldots \quad 20.$$

Whence

$$2n'R' = R + R' - \rho \ldots\ \ldots\ \ldots \quad 21.$$

For $s$ we have

$$\frac{1}{s} = \frac{f'}{y + f'} \cdot \frac{r - x}{r} \cdot \frac{1}{z + \dfrac{yf'}{f + y'}}$$

$$\frac{f'}{yf' + zy + f'z} \times \frac{fyz + ff'\,(y + z)}{xy\,(f + f' + z) + ff'\,(x + y + z) + fyz + f'xz}$$

$\therefore$ by expression for $\rho'$

$$\frac{\rho'}{s} = \frac{f'}{z\,y + f'\,(y + z)}\;\frac{f\,y\,z + f\,f'\,(y + z)}{l\,(f + f' + z) - z^2}$$

$$= \frac{f\,f'}{l\,(f + f' + z) - z^2}$$

$$= \frac{R + R' - \rho}{2\,l - 2\,(l - \rho)} = \frac{R + R' - \rho}{2\,\rho} \quad \ldots \quad \ldots \quad 22.$$

Whence by symmetry $s = s'$, or the current flowing through the end B when a battery is applied to A is the same as that flowing through the end A when the battery is applied to B.

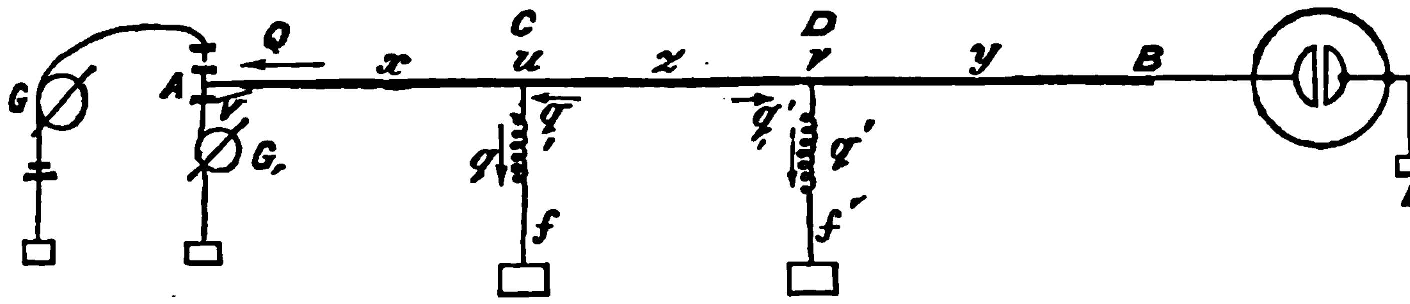
Collecting the equations they may be written

$$\left.\begin{aligned} &\frac{\mu - 1}{\mu + 1} = \frac{R' - R}{\rho} \\ 4\,R\,r' = 4\,R'\,r = 4\,\rho\,\rho' &= 2\,s\,(R + R' - \rho) = 2\,s'(R + R' - \rho) \\ &= 4\,s\,R\,n = 4\,s\,R'\,n' = 4\,R\,R' - (R + R' - \rho)^2 \\ &= 2\,l\,(R + R') + 2\,(R' - R)\,(x - y) - 2\,\rho\,z - 4\,x\,y \end{aligned}\right\} 23.$$

The nine equations, 23, are the result of eliminating $f$ and $f'$ from equations 1 to 11, and give eight equations of identity between the observed quantities R, R$'$, $\rho$, $\rho'$, $n$, $n'$, $s$, $s'$, $r$, $r'$, $\mu$, and one relation only between $x$ $y$ $z$ and these quantities. In practice the equations of identity would not hold accurately, because in forming them it is supposed that the resistance of the faults remains constant, a condition that would not hold in practice, for the resistance of a fault is commonly a function of the strength and direction of the current by which it is measured, and both these quantities would vary in measuring $\rho'$ and $\rho$, R and $\mu$ &c.

There remains the method of discharge. To this method the objections above given apply with much less force, because all that is needful is that the resistance of the faults remain constant during the time of discharge, a quantity not exceeding (unless very great accuracy is aimed at) a few seconds, in the case of the longest cable.

Let connections be arranged as below:




At A a key connects the cable first with a battery through the galvanometer G, then with earth through the galvanometer $G_1$. The key A has a very powerful spring, changing the contacts in a very small fraction of a second.

The deflection on G measures the resistance of the circuit.

The throw on $G_1$, the discharge from the cable.

$f, f'$ are the resistances of the faults at C and D respectively.

$u, v$ are the potentials of the faults at C and D respectively.

$q, q'$, the quantity of electricity discharged at C and D respectively.

Q the quantity discharged at A.

$q, q'$, the quantities discharged from the portion of the circuit.

C, D at the points C and D.

$n.$ V is the potential at B measured by an electrometer or otherwise. The rest of the notation as before.

By measuring at the same, or nearly the same instant, the quantities Q, R, $n$, three relations between these quantities and $f, f'$ are given, by eliminating $f, f'$ there results one relation between $x, y, z$. By repeating the measurements, the conditions of the ends being reversed, a second relation between $x, y, z$ is given, and these combined with $l = x + y + z$, fix the position of each fault.

If $v'$ is the potential at any point at the time $t$ distant from one end of a portion of cable by a length, the resistance of which is $x'$, and if R is the resistance and C the capacity of the cable per unit length, if $\gamma'$ is the current at the same point measured in the same direction as $x'$, and if $u$, $v$ are the initial and $o$ the final potentials at the extremities corresponding to $x' = o$, and $x' = l$, then, as usual, the current being supposed constant at first.

$$\frac{d^2 v'}{d x'^2} = \frac{c}{\mathrm{R}} \frac{d v'}{dt}, - \gamma = \frac{d v}{d x'}.$$

Integrate twice with regard to $x'$ first from $x' = x$ to $x$, secondly from $x = l$ to $x = o$, and then with regard to $t$, $t = \infty$ to $t = o$, we obtain

$$\int_o^\infty (\underset{x=l}{v'} - \underset{x=o}{v'} + \underset{x=o}{l\gamma})\, dt = -kc\int_o^l\int_o^x \left\{u + \frac{x'}{l}(v - u)\right\} dx$$

$$= -\frac{c\,l^2}{6\,k}(v + 2\,u) \quad \ldots \quad \ldots \quad \ldots$$

Applying this equation to the sections of cable of resistance and remembering that $\int_o^\infty u\,dt$ is $f\,q$, for example, we find

$$f\,q - x\,Q = -\frac{1}{6}x^2\frac{c}{k}(2\,V + u) \quad \ldots$$

$\therefore$ $v'$ vanishes except when $t = o$.

$x = o$.

$$f'\,q' - f\,q + z\,q_1 = -\frac{1}{6}z^2\frac{c}{k}(2\,u + v) \quad \ldots$$

$t$ the resistance of $G_1$ being neglected.

Again, because the cable is totally discharged,

$$Q + q + q' = \frac{c}{k}\left\{c\,v\,y + \frac{1}{2}(u + v)\,z + \frac{1}{2}(u + V)\,x\right\}$$

$$q_1 + q_2 = -\frac{1}{2}\,\frac{c}{k}\,z\,(u + v) \quad \ldots \quad \ldots$$

because the section $z$ is discharged.

$$q' + q_2 = v\,y\,\frac{c}{k} \quad \ldots \quad \ldots \quad \ldots \quad \ldots$$

because the section $y$ is discharged.

(27) + (28) — (29) gives

$$Q + q + q_1 = \frac{1}{2}x\,(u + V)\,\frac{c}{k}.$$

Substituting for $q_1$ and $q'$ in equation 26,

$$Q\,(z + f') + q\,(f + f' + z) = \frac{1}{6}z^2\frac{c}{k}(2\,u + v)$$

$$+ f\left\{v\,y + \frac{1}{2}(u + v)\,z + \frac{1}{2}(u + V)\,x\right\}\frac{c}{k}$$

$$+ \frac{1}{2}x\,z\,\frac{c}{k}(u + V) \quad \ldots \quad \ldots \quad \ldots$$

Eliminating $q$ between equations 25 and 30,

$$Q \cdot \frac{k}{c} \left\{ x(f + f' + z) + f(z + f') \right\}$$

$$= \frac{1}{6} x^2 (f + f' + z)(2V + u) + \frac{1}{6} z^2 f (2u + v) + \frac{1}{2} f x z (u + V)$$

$$+ f f' \left\{ v y + \frac{1}{2}(u + v) z + \frac{1}{2}(u + V) x \right\} \qquad 31.$$

We have now to express $f, f', u, v$ in terms of ...... R, $n$ and V.

By what precedes,

$$R = \frac{f f' + f(x + z) + f' x + x z}{f + f' + z} = x + \frac{f(f' + z)}{f + f' + z}$$

$$n = \frac{f f'}{f f' + f(x + z) + f' x + x z} = \frac{f f'}{F} \text{ say.}$$

$$\therefore R n = \frac{f f'}{f + f' + z}$$

$$\therefore R(1 - n) - x = \frac{f z}{f + f' + z}$$

$$z - R(1 - n) + x = \frac{z(f' + z)}{f + f' + z}$$

Whence

$$\frac{z(R - x)}{z - R(1 - n) + x} = f$$

$$\frac{R n z}{R(1 - n) - x} = f'$$

$$n F = f f'$$

$$\frac{z(R - x)}{R(1 - n) - x} = f' + z$$

$$u = \frac{R - x}{R} V$$

$$v = n V.$$

To simplify the notation assume that Q is the quantity of electricity discharged by a length of cable of resistance $a$, charged to a potential V.

Then, substituting in equation (31.), and multiplying by 6,

$$6\,a\,\mathrm{QF} = 6\,a\,.\,ff'\frac{1}{n}$$

$$= x^2\,(f + f' + z)\frac{3\,\mathrm{R} - x}{\mathrm{R}}$$

$$+ z^2\,.\,f\,.\,\frac{2\,\mathrm{R} + \mathrm{R}\,n - 2\,x}{\mathrm{R}}$$

$$+ 3\,ff'\left\{2\,n\,y + \frac{\mathrm{R}\,(1 + n) - x}{\mathrm{R}}\,z + \frac{2\,\mathrm{R} - x}{\mathrm{R}}\,x\right\}$$

$$+ 3\,f\,x\,z\frac{2\,\mathrm{R} - x}{\mathrm{R}}.$$

Dividing this equation by $f\,f'$ and substituting for the functions of $f$ and $f'$.

$$6\,\frac{a}{n} = x^2\,\frac{(\mathrm{R} - x)\,z^2}{\mathrm{R}n\,z^2\,(\mathrm{R} - x)}\,.\,\frac{3\,\mathrm{R} - x}{\mathrm{R}}$$

$$+ z^2\frac{(\mathrm{R}\,\overline{1 - n} - x)\,(\mathrm{R}\,\overline{n + 2} - 2\,x)}{\mathrm{R}^2\,n\,z}$$

$$+ 6\,n\,y + 3\,(1 + n)\,z - 3\,\frac{x\,z}{\mathrm{R}} + 6\,x - 3\,\frac{x^2}{\mathrm{R}}$$

$$+ 3\,x\,z\,\frac{2\,\mathrm{R} - x}{\mathrm{R}^2\,n\,z}\,(\mathrm{R}\,\overline{1 - n} - x).$$

Rearranging the terms

$$6\,\frac{a}{n} = x^2\,\frac{3\,\mathrm{R} - x}{\mathrm{R}^2\,n} + 6\,n\,y + 6\,x - 3\,\frac{x^2}{\mathrm{R}}$$

$$+ 3\,x\,\frac{2\,\mathrm{R} - x}{\mathrm{R}^2\,n}\,(\mathrm{R}\,\overline{1 - n} - x)$$

$$+ z\,\frac{(\mathrm{R}\,\overline{1 - n} - x)\,(\mathrm{R}\,\overline{2 + n} - 2\,x)}{\mathrm{R}^2\,n}$$

$$+ 3\,z + 3\,n\,z - 3\,\frac{x\,z}{\mathrm{R}}$$

$$= 6\,n\,l + \frac{x}{\mathrm{R}^2\,n}\left\{6\,\mathrm{R}^2\,(1 - n^2) - 6\,\mathrm{R}\,x - 2\,x^2\right\}$$

$$+ \frac{z}{\mathrm{R}^2\,n}\left\{\mathrm{R}^2(2 + 2\,n - 4\,n^2) - 2\,\mathrm{R}\,x\,(2 + n) + 2\,x^2\right\}$$

Dividing by 2, and putting the term $6\,n\,l$ on the left-hand side,

$$3\,\frac{a - n^2 l}{n} = \frac{z}{R^2 n}\left(R\,\overline{1 - n} - x\right)\left(R\,\overline{1 + 2\,n} - x\right)$$

$$+ \frac{1}{R^2 n}\left\{R^3 - (R - x)^3 - 3\,R^2\,n^2 x\right\}$$

or

$$z = \frac{3\,(a - l\,n^2)\,R^2 - R^3 + 3\,R^2\,n^2 x - (R - x)^3}{(R\,(1 - n) - x)\,(R\,(1 + 2\,n) - x)} \qquad 32$$

In like manner by observing the discharge and resistance from the end B, and the potential at the end A, assuming $a$ R, $n$ to become $a'$, R′, $n'$.

$$z = \frac{3\,(a' - l\,n'^2)\,R'^2 - R'^3 + 3\,R'^2\,n'^2\,y - (R' - y)^3}{(R'\,(1 - n') - y)\,(R'\,(1 + 2\,n') - y)} \qquad 33.$$

from equations 32 and 33 $x$ and $y$ can be determined.

We might substitute for $z$ from equation 32 in equation 33, and form an equation in $x$ only, but the equation is of the seventh order, and would be very troublesome to solve numerically.

Instead of this it is not difficult to solve the equations by trial.

Assume $$\frac{R - x}{R} = s$$

and $$z = \zeta\,R$$

then $$\zeta = \frac{3\,\dfrac{a - l\,n^2}{R} - 1 + 3\,n^2 - 3\,n^2 s - s^3}{(s - n)\,(s + 2\,n)} \quad \ldots \quad 34.$$

$s$ must be less than unity, and not less than $n$ where $n$ is less than unity.

The process of solution would be as follows :—

Assume for $s$ first the value $\dfrac{1 + n}{2}$ and calculate the corresponding value of $\zeta$ in equation 34.

then $$x = R\,(1 - s),\ z = \zeta\,R$$

whence $$y = l - R\,(1 - s) - \zeta\,R.$$

Substitute this value of $y$ in equation 33, and calculate the corresponding value of $z$ if this value is the same as the value found for $z$ from equation 34, the problem is solved, if it is $\begin{cases}\text{greater}\\\text{less}\end{cases}$

than that from equation 34 then $y$ is two $\left\{\begin{matrix}\text{great}\\ \text{small}\end{matrix}\right\}$ and $x + z$ too $\left.\begin{matrix}\text{small}\\ \text{great}\end{matrix}\right\}$ and because in equation 32 the greater $x$ is the greater $z$ is it follows that a $\left\{\begin{matrix}\text{higher}\\ \text{lower}\end{matrix}\right\}$ value of $x$ must chosen. Try next then

$$\frac{1}{2}\left(n + \frac{1}{2}(1 + n)\right) \text{for } s$$

$$\frac{1}{2}\left(1 + \frac{1}{2}(1 + n)\right) \text{for } s$$

and so on on each trial, halving the interval between the last value of $s$ tried, and either $n$, or unity, as the value of $z$ in equation 33 is greater or less than that from equation 34, until two values are found between which $s$ lies when the intervals between successive values tried may be halved.

In this way a few trials will give $x$, $y$, $z$ with considerable accuracy, and with no very great amount of arithmetical labour. In practice, of course, the process will be shortened often by previous knowledge or by the inspection of the values of $z$ found from the two first trials, indicating a better value of $s$ to try for the third trial than that given by the foregoing rule.

As to the practical details of the observations.

The potentials at A and B must be measured in terms of a known standard of electro-motive force, suppose a Clark's cell. The methods of doing this are too well known to need more than a reference to them here.

The effect of earth currents must be eliminated in the measurement of R by reading to a "false zero" on the galvanometer G,

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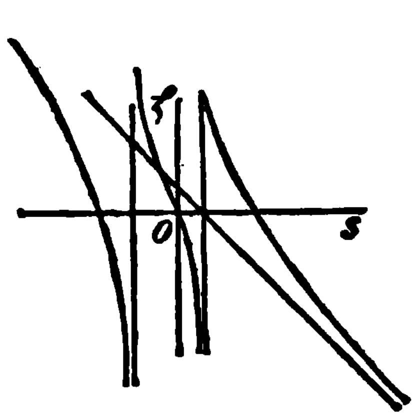



That but one value of $x$, $y$, $z$ can satisfy these equations within the possible limits may be shown thus:—

Treating $s$ and $\zeta$ as co-ordinates, equation 32 represents a curve of the 3rd degree with a double point at infinity. The asymptotes are $S = n$, $S = -2n$, $s + \zeta = n$. Again, $\zeta$ is negative for values of $s$ just less than $n$, and positive for values a little greater than $-2n$. The curve is therefore of a shape something like this, and it is clear that but one value of $s$ corresponds to one value of $\zeta$ outside the asymptote $= n$.

their effect on the electrometer at B by the same means, the potentials just before and just after the discharge being noted and the difference given as the potential at B.

To eliminate the effect of earth currents on $G_1$ is much more troublesome, and on a short cable of 50 or 100 miles in length might prove impossible, but in such case the resistances of the faults would be small also, and R and R′ would be fair approximations to $x$ and $y$ respectively.

The general method is as follows:—

G has a needle swinging freely, so that once set in motion it will continue to vibrate many times.

The observations may be made as follows:—

After discharging the cable retain the same shunt for the galvanometer, and disconnecting the instrument from the cable set the needle in motion. Observe any deflection $\delta$, and also a second deflection $\delta'$, some time afterwards, the needle having performed $n$ oscillations from rest to rest during the interval. Calculate $\frac{1}{n} \cdot \log._e \frac{\delta}{\delta'}$ and call the result $z_1$.

Before this observation is made, and when discharging the cable note not only the first throw but also the return throw when the needle again comes to rest, and the third deflection also.

Let $\theta_1$, $\theta_2$, $\theta_3$ be the deflections observed.

Calculate the quantity

$$\frac{\theta_1\,\theta_3 - \theta_2^2}{\theta_1 + \theta_3 - 2\,\theta_2} \quad \dots \quad \dots \quad \dots \quad \dots \quad 35.$$

(where $\theta_2$ is reckoned *positive* when on the same side of zero as $\theta$ and $\theta_3$), the result is the deflection due to the permanent earth current which we may call $\phi$ (generally $\theta_2$ will be negative).

Then if $\theta$ is the true discharge due to the charge in the cable only, and independent of the earth current.

$$\zeta \,.\, \theta = \frac{z_1\,\phi}{\sqrt{z_1^2 + \pi^2}} + \sqrt{(\theta_1 - \phi)^2\, e^{2\chi} - \frac{\pi^2\,\phi^2}{z^2 + \pi^2}} \quad 36.$$

Where

$$\chi = \frac{z_1}{\pi} \tan.^{-1} \frac{\pi\,\theta\,\zeta}{z_1\,\theta\,\zeta - \phi\,\sqrt{z_1^2 + \pi^2}} \quad \dots \quad 37.$$

and

$$\zeta = e^{\frac{z_1}{\pi} \tan.^{-1} \frac{\pi}{z_1}} \quad \ldots \quad \ldots \quad \ldots \quad .. \quad 38.$$

These equations are somewhat complex and the value of $\theta$ could be found from them only by successive approximation.

The method by which they are obtained does not need to be printed in full as it is well known. The resistance of the air is supposed to vary directly as the angular velocity of the needle, an equation results of the form

$$A \frac{\delta^2 \theta}{\delta t^2} = \lambda \gamma - \mu \theta - 2 K \frac{\delta \theta}{\delta t}$$

Whence

$$\theta = \frac{\lambda \gamma}{\mu} + c.e^{n_1 - t} \sin. (n_1 t + \beta)$$

The values of $\theta$ and $\frac{\delta \theta}{\delta t}$ are known when $t = o$, in terms of A, $\mu$, and $k$; thence C and B are known, and $\theta_1$, $\theta_2$, $\theta_3$ can be found in terms of $\phi$, and so $\theta$.

In practice $z$ can be made so small that $z^2$ may be neglected. Then equation (36) takes the form

$$\left.\begin{array}{l} \theta = \psi + z_1 \left( \frac{\phi}{\mu} + \frac{1}{2} \psi + \frac{\phi^2}{\psi} - \frac{\phi^2 + \psi^2}{\psi} \cdot \frac{1}{\pi} \cdot \tan.^{-1} \frac{\psi}{\phi} \right) \\ \text{where} \qquad \psi = \sqrt{\theta_1 (\theta_1 - 2 \phi)} \end{array} \right\} 39.$$

Hence $\theta$ being known, it may be compared with the discharge obtained from a condenser of known capacity, and so the values of $\zeta$ and $a$ above given may be calculated, and $x$ and $y$ found.

Collecting the formulæ, observe R, $n$, $\theta_1$, $\theta_2$, $\theta_3$, $z_1$, and afterwards R′, $n'$, $\theta'_1$, $\theta'_2$, $\theta'_3$, $z_1'$.

Calculate

(1) $\dfrac{\theta_1 \theta_3 - \theta_2^2}{\theta_1 + \theta_3 - 2 \theta_2} = \phi$

(2) $\sqrt{\theta_1 (\theta_1 - 2 \phi)} = \psi$

(3) $\theta = \psi + z_1 \left( \frac{\phi}{\pi} + \frac{1}{2} \psi + \frac{\phi^2}{\psi} - \frac{\phi^2 + \psi^2}{\psi} \frac{1}{\pi} \tan.^{-1} \frac{\psi}{\phi} \right)$.

In like manner calculate $\phi'$, $\psi'$, $\theta'$.

(4.) Taking a discharge from a condenser of known capacity

charged to the same potential as that which existed at the end A of the cable just before it was discharged, calculate ($a$) the resistance of a length of cable such that, charged to the potential abovementioned, it would produce on the galvanometer G (shunted as when the cable was discharged) the deflection $\theta$ just found.

(5.) Find by trial a value of $x$ such

$$\frac{3\,\dfrac{a - l^1 n^3}{R} - 1 + \dfrac{3\,n^2}{R}\,x - \left(1 - \dfrac{x}{R}\right)^3}{\dfrac{1}{R}\left(1 - n - \dfrac{x}{R}\right)\left(1 + 2\,n - \dfrac{x}{R}\right)} = z$$

$$= \frac{3\,\dfrac{a' - l'\,n'^3}{R'} - 1 + \dfrac{3\,n'^2\,y}{R'} - \left(1 - \dfrac{y}{R'}\right)^3}{\dfrac{1}{R'}\left(1 - n' - \dfrac{y}{R'}\right)\left(1 + 2\,n' - \dfrac{y}{R'}\right)}$$

Where $x + y + z = l$, or $y = l - x - z$, remembering that if the first term of these equations is greater than the third too large a value of $x$ has been chosen, if the first term is less than the third too small a value of $x$ has been chosen.

When a value has been found such that the first and third terms of the equations have the same value, the true values of $x$, $z$, $y$ are found.

The PRESIDENT: The paper is one of the kind which it is difficult to follow from a mere reading, and no doubt other members beside myself will prefer to await its publication in the Society's Journal to making any remarks upon it this evening, and therefore I will simply propose a vote of thanks to Mr. Hockin, for his able paper.

# ON TESTING BY RECEIVED CURRENTS.

By H. R. Kempe (Member).

The good maintenance of a system of telegraph lines c be accomplished in a satisfactory manner by a good system ing. The need of some system is unquestionable, but no l be laid down as to what is the best method to adopt. C stances must determine what arrangements are most desirab are likely to prove most satisfactory in any particular case.

In a recent work, entitled "Testing Instructions," Mr. Schwendler stated in the preface that "During my stay in E I have had many opportunities to watch the working of othe graph administrations, and have been surprised to find how progress testing has made. I am always told: "Yes, we s like to introduce a general system of testing; we know its practical utility, but show us a system to do it, and whic work satisfactorily." Mr. Schwendler, in his work, has des the principles of the system adopted in India, and a very elab system it is, but with the highly educated telegraphic sta whom the Indian telegraphic administration is carried on, it the same time quite a successful system, and one which is, no d quite necessary in a country like India, where the telegraph sta are comparatively few and far between, and where the localis of a fault within the limits of a stretch of line several hun miles long is often necessary.

In European countries, and in England in particular, such elaborate system involving the use of intricate formulæ, is nei desirable nor requisite, and nothing would be gained by adoption of such methods of testing: had such methods be necessary in the Postal Telegraph Department they would h been put into force, and their non-adoption must be taken a signification of their unsuitableness for the maintenance of telegraph lines in this country.

For some years past a simple, and in many respects a ve satisfactory system of testing has been in force in the Post Telegraph Service; lately, modifications have been introduc

## Engineer's Department.

| Date of Testing. | Terminal Stations of Circuit (Names to be given in full). | | Section Tested. | | Road, |
|---|---|---|---|---|---|
| | Up Stations. | Down Station. | From | To | |
| April 19th ... | Bristol | Gloucester ... | Bristol ... | Gloucester ... | Rail |
| „ 9th ... | „ | Cardiff ... ... | „ ... | Cardiff ... ... | „ |
| „ 12th ... | „ | Cardiff H.P.O. | „ ... | Cardiff H.P.O. | „ |
| „ 9th ... | „ | Swansea ... | „ ... | Swansea ... | Rail |
| „ 9th ... | „ | Cheltenham | „ ... | Cheltenham... | |
| „ 1st ... | 159 | | Gloucester | North Leach | Road |

which have given still more satisfactory results, and it is of this improved method that I purpose chiefly to speak this evening.

Tests may be divided into two groups, viz., "Testing for actual faults," and "Testing to ascertain the general condition of a line." To detect the locality of faults no elaborate tests are required or used in this country. The lengths of line between any two stations in the United Kingdom are comparatively short, varying from 15 to 20 miles, and therefore the localization of a fault between any two stations *en route* of the line is really all that is necessary to ensure its speedy removal, and this is done by causing each station in succession to disconnect or put to earth the wire at his test box, a test being made by sending a current through a galvanometer to the line as each station disconnects or puts to earth, until it is seen that the faulty section is arrived at or past, when the lineman is advised, and he immediately examines the section in question. The tests made to ascertain the condition of the line are of two kinds, viz., "Monthly Tests" and "Daily Tests," the former are tests made with the ordinary Wheatstone bridge, and consist of "Insulation" and "Conductivity" tests; the insulation being taken with zinc to line, and the conductivity first with zinc to line and then with copper to line. In the latter case the arithmetic mean of the two measurements gives approximately the correct resistance of the line, since the two values never differ greatly.

The tests are entered on sheets, specimens of which are here shown. In working out the results from the measurements, certain deductions are made for the resistances of instruments and underground work which may be in circuit at the time the tests are taken.

The monthly tests are very useful for detecting incipient faults either in insulation caused by broken insulators, &c., or in conductivity caused by defective joints in the wire.

To enable the tests with reversed currents to be rapidly made, the Wheatstone bridge (a specimen of the form of instrument used was exhibited) is provided with a reversing switch, which, unlike the usual practice, does not reverse the battery, but reverses "line" and "earth;" by this device the galvanometer deflection

due to, say, too much resistance being inserted in the arm of the bridge, is always on the same side, although the direction of the current through the line may be reversed; thus, the inspector making the test can easily see, without chance of a mistake, whether his balance is out in consequence of too much or too little resistance being inserted in the arm of the bridge.

The "Daily" tests were formerly insulation tests, made by means of the tangent galvanometer. A constant deflection was first taken with 1000 ohms in the circuit of the galvanometer and testing battery, and then the resistance being removed, the line, whose further end was insulated, was inserted in its place, and the new deflection noted. From these tests the insulation resistance was calculated by direct proportion, or ascertained directly by means of a table worked out for the purpose. In this table, the first vertical column represents the deflections obtained with the 1000 ohms in circuit, and the top horizontal column represents the deflections obtained with the line in circuit; the figures at the point of intersection of the vertical and horizontal columns corresponding to the constant and line deflections, give the insulation resistance of the line.

These tests, although they answered fairly well in practice, were not all that could be desired. A broken wire which did not make earth would show a very high result for insulation, but it would not show the working condition of the wire. To obviate such and other erroneous results, the system I am about to describe was devised.

In 1878, Mr. W. H. Preece brought before this Society a paper on the "Measurement of Currents." In this paper he drew attention to the practice in the United States of measuring the strength of currents in webers. The employment of the unit of current, it was pointed out, is extremely useful, and it is a matter of great surprise that its value was not thought of before. Mr. Preece also explained in his paper how extremely simple was the measurement of currents in terms of the unit weber; he, moreover, suggested the use of a submultiple of the unit—namely, the milliweber or the $\frac{1}{1000}$th part of a weber. This multiple being of extremely convenient dimensions, has now been very generally adopted.

Let us now see in what way the measurement of currents can be made use of for testing purposes. If we take an ordinary telegraph instrument, say a Morse printer, and we connect a 10-cell Daniell battery to it, together with a resistance coil, and then we work the instrument, and go on adding resistance in the circuit until the instrument ceases to respond, then we can see the minimum current which can be employed for working purposes. Now, the instrument in question will work well with a 10-cell Daniell in circuit with a total resistance of 3000 ohms; that is to say, it will work well with a current of

$$\frac{10}{3000} = \cdot 0033 \text{ webers or } 3\cdot 3 \text{ milliwebers.}$$

A less current than this will not give satisfactory results.

A telegraph line whose insulation is perfect will have the same amount of current flowing throughout every point of its length, that is to say, the current flowing in at one end will be equal to the current flowing out at the other. Supposing then that we have a line of 1000 ohms resistance, and an instrument at the further end, whose resistance is 500 ohms, and which requires 5 milliwebers of current to work it, then the electro-motive force of the current required to work this instrument, the insulation of the line being regarded as perfect, will be given by the expression

$$3\cdot 3 = \frac{E}{1000+500} \times 1000$$

$$\text{or } E = \frac{1500 \times 3\cdot 3}{1000} = 4\cdot 995 \text{ volts.}$$

Now a Daniell cell has practically an electromotive force of 1 volt, hence a 10-cell Daniell battery will give 10 volts, or will produce on the instrument a signal due to a current exceeding 3·3 milliwebers, and therefore will give good signals provided there be no leakage, or very little, on the line; if, however, the leakage be considerable the 10 cells will not prove sufficient for the purpose.

The good working of a line is dependent upon the receipt at the receiving end of a sufficient current to work the instrument properly; it is of no avail, if plenty of current goes out, if but little arrives at the end to work the instrument there.

The current going out can be measured or estimated, and the current received can be measured, and the relative proportion

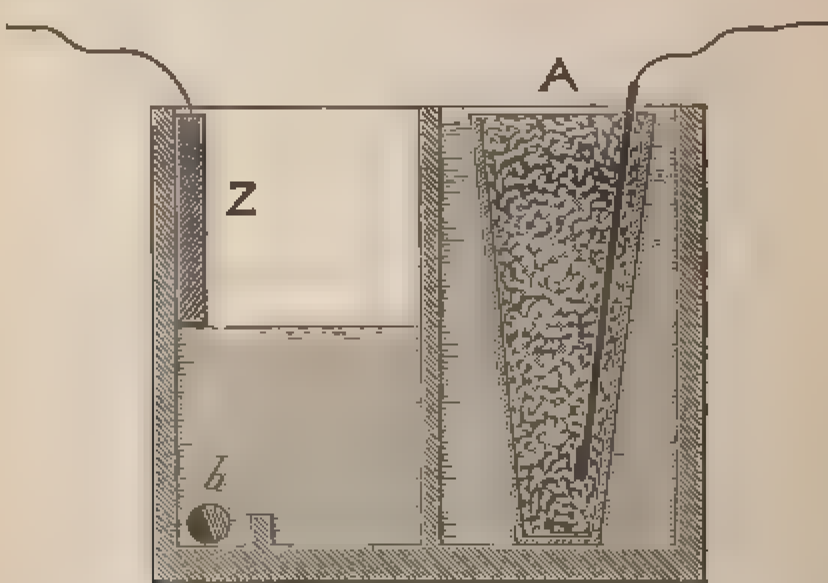
of these quantities indicates the working condition of the line and batteries.

Supposing the line and batteries to be in good condition, then we know that the strength of the outgoing current is given by dividing the electromotive force of the battery by the total resistance in the circuit—the result obtained is the greatest current that can be received at the end of the line; in the system of testing adopted in the Postal Telegraph Service, this value is the standard with which the received currents are compared. We know what the resistances of the lines are, we know the battery powers sending the currents, consequently we know the currents that ought to be received; and as the daily measurements show the actual currents received, we have thus a record which shows the working condition of the lines.

The measurement of the currents is effected by means of the tangent galvanometer and standard cell here shown. (A specimen of these instruments was exhibited).

The standard cell is formed with two chambers, as shown in Fig. 1.

Fig. 1.




The right hand compartment is called the "idle" chamber, as in it is placed the porous pot A, containing a copper plate and sulphate of copper crystals, when the cell is not required for use. The left hand compartment is kept filled to a level with the zinc plate Z, with a half-saturated solution of sulphate of zinc. A zinc rod *b* lies in a small compartment immediately below the zinc plate Z.

When the cell is required for use, the porous pot and its contents is removed from the right hand chamber and placed in the left hand chamber, its bulk causing the liquid in the latter to rise and cover the zinc plate Z; the cell is then ready for use. When the cell is no longer required for use, A is again placed in the right hand chamber, and whilst the cell is at rest any sulphate of copper solution which may have become mingled with the sulphate of zinc solution, has its copper decomposed and deposited on the zinc rod *b*; thus the solution is always kept clear.

FIG. 2.

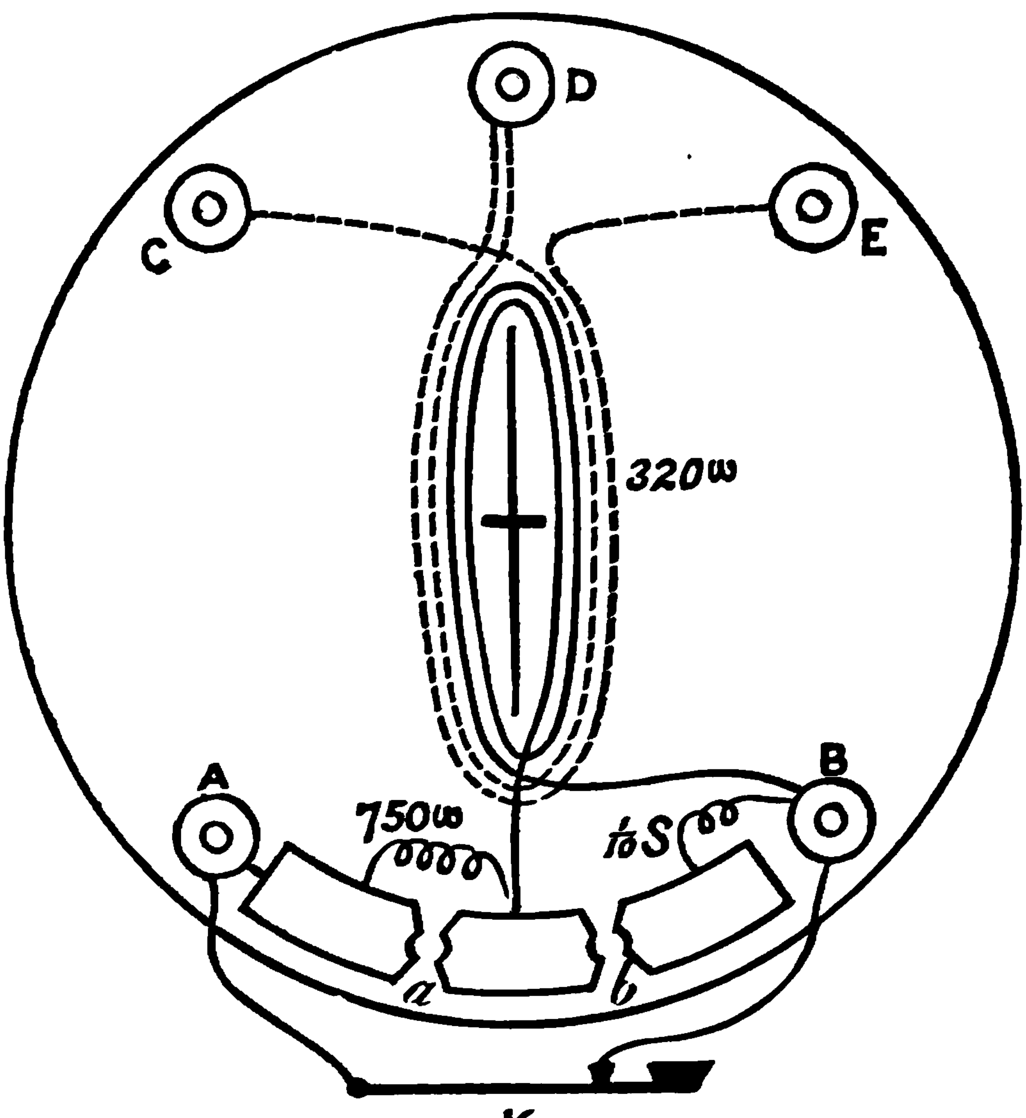



Fig. 2 shows in general plan the form of tangent galvanometer used for making the measurement of the currents.

In this instrument there are three coils of wire, the one nearest the needle consisting of No. 35 copper wire wound to a resistance of 320ω. The other two coils are of No. 18 gauge—the one between C and D making three turns, and the one between D and E making twelve turns in the opposite direction. The latter coils are for testing batteries. Calling the degree of sensitiveness obtained with the terminals C and D 1, then the degree of sensitiveness obtained with terminals C and E will be 3, and that with terminals D and E will be 4; if C and E are coupled and the connecting wires from the battery are joined t

then a degree of sensitiveness equal to 1·6 will be obtained. The resistance of the wires is practically *nil.*

In testing the strength of a current in milliwebers, the standard cell is connected to A and B, and both plugs being removed from the plug-holes *a*, *b*, the key K is depressed. There is then in circuit a total resistance of 1070$^\omega$, viz., 750 + 320. As the electromotive force of the standard cell is 1·07 volts, the resulting deflection of the Galvanometer-needle (which is about 30°) will be due to a current of

$$\frac{1 \cdot 07}{1070} = \cdot 001 \text{ weber, or 1 milliweber,}$$

and any other deflection obtained with any particular current, compared by direct proportion with the standard deflection, will give the strength of that current in milliwebers. As the currents to be measured are usually in excess of 1 milliweber, the readings are taken with the right hand plug ($\frac{1}{10}$th Shunt) inserted, which is allowed for in the calculation.

When the standard deflection is noted, the standard cell is removed and the circuit from which the received current is to be measured is connected to terminal A, terminal B being put to earth.

The 750$^\omega$ resistance is not plugged out, for the following reason :—if the line tested has a fault of high resistance close to the testing station, then, since the shunted coil has only a resistance of $\frac{320}{10} = 32$ ohms, practically all the received current will pass through it, and it would then appear as if the fault did not exist; by including the 750$^\omega$ in the circuit a diminution in the current must always be caused by the fault.

To save time and trouble, the currents corresponding to the deflections from the received currents are obtained from a table calculated for the purpose. In this table the first vertical column represents the deflection obtained from the standard cell, and the top horizontal column the deflections obtained with the received currents, the figures at the point of intersection of a vertical with a horizontal column gives the current corresponding to those deflections.

The tests when taken are entered upon the form shown—

# POST OFFICE TELEGRAPHS.

ENGINEER'S DEPARTMENT,
SCOTLAND WEST District.

## MORNING TESTS.

GLASGOW Office.

Week ending 24*th April*, 1880.

| CIRCUIT. | Section Tested. From | Section Tested. To | Length of Section. Miles. | Gauge of Wire. | Route. | No. of Cells used. | Estimated Currents Received when Insulation is perfect. Milliwebers. | Observed Received Currents. Milliwebers. Monday. | Tuesday. | Wed'day. | Thursday. | Friday. | Saturday. |
|---|---|---|---|---|---|---|---|---|---|---|---|---|---|
| G.W. A.B. 1 | G.W. | A.B. | 156¼ | 6 & 8 | Road & Rail | 30 | 9·22 | 6·48 | 7·22 | 5·75 | 7·98 | 7·98 | 7·60 |
| „ „ 2 | „ | „ | 159½ | 8 | „ | „ | 8·84 | 5·75 | 6·48 | 4·34 | 7·22 | 7·60 | 6 48 |
| „ I.V. ... | „ | I.V. | 211 | 8 | „ | 80 | 15·63 | 10·86 | 12·21 | 10·43 | 12 21 | 12·21 | 11·30 |
| „ O.B.... | „ | O.B. | 97¼ | „ | „ | 60 | 14·61 | 11·30 | 11·75 | 8·77 | 12·69 | 12·21 | 11·30 |
| „ C.R.X. | „ | C.R.X. | 128 | „ | „ | 50 | 15·24 | 7·98 | 10·00 | 7·22 | 10·86 | 10·86 | 10·00 |
| „ B.E.... | „ | S.X.W. | 96¾ | „ | „ | 20 | 8·77 | 7·60 | 7·60 | 6·85 | 7·98 | 7·60 | 7·22 |
| „ L.D.... | „ | „ | „ | „ | „ | „ | 7.57 | 6·85 | 6·48 | 5·39 | 6·85 | 6·85 | 6·11 |
| „ D.N. 1 | „ | „ | „ | „ | „ | „ | 8·36 | 7·22 | Earthy | 6·48 | 7·60 | 7·22 | 6·85 |
| „ „ 2 | „ | „ | „ | „ | „ | „ | 8·26 | 7·22 | 7·22 | 6·11 | 7·60 | 7·22 | 6·85 |
| Weather... ... ... ... ... ... ... ... ... ... | | | | | | | ... | Damp. | Damp. | Heavy Rain. | Damp. | Fine. | Wet. |

REMARKS.

Signature ____________________

It may be noticed that the "Estimated" values of the currents differ for those wires which are of the same length and gauge; this is due to the fact that, although the gauges are uniform the resistances are not the same.

Mr. J. A. BETTS: I quite agree with Mr. Kempe that it was impossible to introduce elaborate tests into England, owing to the large number of wires to be tested in a given short time. I remember that in 1872 I had to test about 32 wires for both insulation and line resistance in 15 minutes. It could be done when the tests came out as we expected them; but if a wire became faulty or in any way troublesome, we either had to pass it by for further investigation, or slur over the remaining tests. Still, for all this, we *did* know the state of our wires, and could detect faults creeping in, and repair them before they became serious. In answer to the President, Mr. Betts said—In China we have been satisfied with the most rough and simple tests as far as telegraphs are concerned.

Professor AYRTON: No doubt systems of testing must differ according to the practical requirements of the country. In India, as explained by Mr. Kempe, a much more perfect system than that required in England is necessary, on account of the great difference in length of the circuits; for it is always easy to find that a fault exists between two stations, but the question is exactly where between these two stations. If the two offices are only a few miles apart, the solution of this question is comparatively easy without resorting to electrical methods; but if they be separated by a couple of hundred miles, with no railway joining them, a process of determining the position of the fault by inspection is a most tedious, uneconomical, and unsatisfactory practice. Mr. Kempe is, I think, quite right in saying that the system of testing adopted in India has been brought to an amazing degree of perfection, thanks to the energy and skill of Mr. Schwendler, and of those working with him. I have known instances of the position of a fault which has come on after dark being accurately determined during the night, and removed, by a man sent out to the spot, before morning.

It is always, of course, a great point, when testing for faults, to know the normal condition of the line, and I think sufficient attention in former times was not generally given to ascertaining periodically the state of telegraph lines when free from faults. It is the same with a telegraph line as with a human being: a

doctor is often only thought necessary after the illness has commenced, whereas the conditions of health must be studied before those of disease; and the family doctor, who knows the patient's constitution well, can often do more good than a single visit to a more famous physician. Now, one of the aims of the complete system of testing employed in India, is to periodically feel the pulse of every telegraph line there belonging to the Government.

The PRESIDENT: It is not for me to say much on this subject. The British public possesses the charming prerogative of finding fault with those who manage its own concerns. Those who are employed in conducting and upholding the business of telegraphy in this country are in the position of being attacked right and left by everybody; and I am sure it gives us great satisfaction to find that an explanation of the system of testing introduced by the Post Office is received by the members of this Society with such favour. I do not think sufficient credit is accorded to those who manage the telegraphs in this country for the manner in which the work is done. During the past few weeks a very exciting time has been passed. The amount of ordinary telegraphic business has been something fabulous, while the extra Press work in consequence of the General Election has been almost incalculable. No hitches in transmission have occurred, and this result has been achieved mainly through the excellent system of testing which Mr. Kempe has explained, and he himself is one of those to whom we are principally indebted for its perfection. Mr. Kempe's table cost much labour, but it is a very useful, reliable, and accurate reference table. The application of this system of testing of the wires in this country day by day and week after week, enables the whole 102,000 miles of wire to be kept in constant working order. Interruptions to communication are nowadays quite scarce with us. Our morning "bill of health" is often as low as one or two faults, and it is occasionally with great satisfaction that I attach my signature to a clean sheet, showing that not a single fault exists in the whole United Kingdom. I propose a vote of thanks to Mr. Kempe for his excellent paper.

The Meeting then adjourned.

The Eighty-ninth Ordinary General Meeting of the Society was held on Wednesday evening, May 12th, 1880, at the Institution of Civil Engineers, 25, Great George Street, Westminster—Mr. W. H. PREECE, President, in the Chair.

The minutes of the last meeting having been read, it was announced that Mr. Charles Harry Stacey had been transferred from the class of Associates to that of Members; the names of new candidates were also read.

The PRESIDENT: I was very grieved last Friday to receive an announcement that Dr. Siemens was too unwell to undertake the arduous duties of preparing his paper on the recent practical applications of the dynamo machine, which had been announced for this evening. But I am glad to say that he has kindly promised to give his paper as soon as possible, and, as the only remaining night of the session is to be occupied by Mr. Edward Graves with his paper on the progress of telegraphy, a special night will be devoted to the purpose.

I have great pleasure in being able to announce to the meeting that the long-promised Ronalds Catalogue is at last completed and ready for distribution to subscribers. A specially-bound copy will be presented to Mr. Carter, the executor of Sir Francis Ronalds. The library will be opened by or before the end of this session for the use of members, and also, under proper regulations, for the use of the public.

When Dr. Siemens communicated the unpleasant fact of his inability to attend to-night, we were rather disconcerted, for the arrangements up to the end of the session had, as we thought, been made, and no papers had been yet sought for next session. To get out of the dilemma I have ventured to undertake to bring before the meeting the following paper, which I trust will prove interesting to every telegraph engineer, viz.,

# THE BEHAVIOUR AND DECAY OF INSULATING COMPOUNDS USED FOR DIELECTRIC PURPOSES.

By W. H. Preece, the President.

During the past few years we have been very much disturbed by a peculiar decay of gutta percha, which has been rather difficult to account for. Every one knows that gutta percha is a gum exuded by a tree found in the Indian Archipelago, called the *Isonandra percha.* This gum is a compound of carbon and hydrogen, containing about 88 per cent. of the former and 12 per cent. of the latter. But it is never found in a pure state, and we have the authority of Professor Abel that it is always found to a certain extent mixed with oxygen and resin. In manufacturing processes these impurities are removed, but others are absorbed which tend to the decay of the material. It is curious to note that this gum is isomeric, or identical in its chemical construction with oil of turpentine and other compounds of that class; but well known as it is to the telegraph engineer, it is quite a child amongst the materials applied to the arts. About 1842 Dr. Montgomery noticed in the hands of a native, in one of the islands of the Indian Archipelago, the handle of a hatchet formed of a very peculiar material, which he was told became plastic in warm water, and capable of being moulded to any shape. He brought a specimen of it to England, and in 1845 brought it before a meeting of the Society of Arts, at which Dr. Siemens was present. The idea was at once suggested to Dr. Siemens that such material had a commercial value for telegraphic purposes. Experiments were made with it by Dr. Werner Siemens, in Berlin, and Faraday pronounced it as possessing electrical qualities of a high order, and a company was formed for bringing it into the market, since when it has been used in almost every conceivable form for electric and other purposes. I need not dwell on its application; my present object is to bring out its durability and behaviour when applied to underground wires.

The gum is soluble in olive oil, benzine, turpentine, and other such spirits; it is attacked by ozone, creosote, and liquids of that

kind. It combines with oxygen, and undergoes the ordinary process of rust, which proceeds with great alacrity in air, especially when also subjected to the influence of light. This combustion by oxygen is also assisted by intermittent exposure to moisture. In process of manufacture gutta percha becomes mechanically united with water, which evaporates on exposure to variations of temperature, and leaves the gum remaining in a dry and brittle condition. This condition led Mr. Edwin Clark to coat gutta percha with tape saturated with tar, in the belief that the tar would replace the water and keep the gum in its elastic state; but, unfortunately, tar contains creosote, which acts on gutta percha in a deteriorating manner, and so the evil remained.

In the years 1852-3-4, a great quantity of gutta percha covered wire was laid down, and some of the gentlemen now present had the responsibility and management of the work. These wires deteriorated, and among the many causes of deterioration was one frequently brought forward for discussion before the Institution of Civil Engineers, viz., the growth of fungus upon the gutta percha wires. Mr. Highton, an old telegraph engineer, who is no longer with us, found that the wires speedily decayed between London and Manchester, especially among the roots of oak trees; and in such places of decay the mycellium of a fungus was always present, and this fungus he set down as an active agent in the decay of gutta percha. I have reason to believe, and will presently show you why, that Mr. Highton was not quite right in his supposition. In all systems of underground wires it has been found that gutta percha wire decays in the neighbourhood of joint boxes—the air in the boxes attacks the gutta percha and dessicates it. This fact has led to the practice, now generally followed in the streets of London and elsewhere, of as far as possible hermetically sealing the joint boxes and excluding the air.

Another agent in the destruction of underground wires is lightning, or the induced currents caused by lightning. The connecting wires between the Central Telegraph Station in London and the various railway termini are laid underground, and, when a current caused by lightning passes through the underground wires to find "earth", if a weak spot in the percha comes in its path, it

escapes through it, developing a fault and causing leakage. To remedy this, lightning protectors are now being fixed at the junction of the underground and overground wires.

Another enemy to the preservation of gutta percha exists in vermin: rats indulge freely in gutta percha, and are evidently only deterred from knawing through it by shocks received on reaching the conducting wire. Mice have contributed their share of interruptions to gutta percha covered wires.

But the peculiar cause of deterioration in gutta percha that I want to bring before you to-night, is one due to the existence of an extremely minute insect—an animal so small that it easily escapes the naked eye. It happened a short time ago that simultaneously underground wires in different parts of the country showed novel signs of decay similar to each other. Not being satisfied with the reason attributed by the local officers, I resolved to examine the wires *in situ* for myself. I used my microscope to examine the dirt surrounding the wires, and soon perceived that an almost invisible white speck was really an insect of a very lively disposition, for as soon as it alighted on one place it jumped off to another. The presence of this animal assured me that I was on the track for tracing out the cause of decay in gutta percha wire; and, as the London street wires were affected, I invited Sir John Lubbock to examine a joint box, and he at once recognised the creature as the *Templetonia crystallina*, belonging to the genus *Podura*, so called because they are distinguished by a little sub-abdominal organ which might be described as a leg or a tail, but it is neither, but it has the peculiarity of giving to its owner the power of springing about with the activity of a flea. (Mr. Preece here pointed to a magnified drawing of the insect, and described its physical construction.) There can be no doubt that this depredator has a taste for gutta percha, and that it is the cause of decay in underground wires, because since its existence has been known it has always been found present at these peculiar points of decay. On close examination of a specimen, a dark substance resembling percha can be seen in the animal's stomach, but that it is percha cannot well be proved, on account of the smallness of the creature. Microscopes and slides with specimens of this

*Templetonia crystallina* are on the table, so that members may ma themselves familiar with this new agent in the destruction gutta percha covered wires. I have every reason to believe, fro similarity of appearances, that this minute insect was the cause the decay that Mr. Highton ascribed to the mycellium of a fungu How is gutta percha to be cured from this effect? At present th only plan that has been practically tried, and which will probab be used for the purpose, is to protect the gutta percha with lea When wires were laid underground in the early days they we covered with lead, but owing to its cost the practice was abandone though it seems clear now that for the proper protection of gut percha in certain places lead will have to be again resorted t By covering the wires with lead, we shall in my mind practicall ward off the enemy by causing him to pass through that materi before be can arrive at the percha.

I have made these remarks in the hope that the general question of the durability and protection of gutta percha may be fully discussed; and, as there are many present who have had great experience in that direction, I shall be glad if they will favour us with their remarks.

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Mr. D. BROOKS (Philadelphia) stated that gutta percha covered wire in America was used solely for river crossings; but he claimed that the idea of so covering wire originated in America, as the law had recently decided against the Western Union Telegraph Company for using wire which came under a patent taken out some fifteen years ago. "Kerite," the composition of which he did not know, was used for covering wire used for instrument connections, etc., but any gutta percha covered wire required in America was always obtained from England.

Mr. TRUMAN asked whether the insect was known before the time at which Sir John Lubbock's opinion was asked?

The PRESIDENT: Certainly.

Mr. TRUMAN: Were its natural habits known before it took a fancy to gutta percha?

The PRESIDENT: Yes. The animal abounds in Kent, under leaves and old decaying vegetable matter.

Mr. TRUMAN: Could it be said that the localities in which the insect had been found in connection with gutta percha were such that the animal might naturally be expected in without the presence of the gutta percha?

The PRESIDENT: We did not go so far as that.

Mr. TRUMAN explained that he asked the question because, with the variety of gutta percha gums, it was possible that a particular kind or a peculiar condition of any kind of gum was specially subject to attack by the insects, or that the same agent which destroyed the percha brought the insect. The treatment of gutta percha in process of manufacture and after use determined the durability of the gums, some of which were more suitable for covering telegraph wires than others.

Mr. LATIMER CLARK: We are all, I am sure, indebted to the President for the interesting matter he has laid before us to-night. I have examined some of the specimens on the table, and I at once recognise the form of decay as one with which I have been familiar in former years, and I agree with Mr. Preece that it is manifestly due to the percha being bitten by insects.

The *Templetonia crystallina* is an insect of the *Podura* class. The little black insects that are seen hopping on the water in the puddles formed by horses' hoofs after rain belong to the same tribe, and the *Podura* itself is often found in cellars. The latter species is well known to microscopists, as the scales which clothe the bodies of these small insects are in constant use as one of the most delicate tests for the microscope.

It is a curious fact that, after so long experience, we have never yet solved the question of protecting gutta percha from decay by oxidation, for this is certainly the real cause of decay in ninety-nine cases out of every hundred. Oxidation is entirely arrested by the immersion of gutta percha in water. Submarine cables never suffer from oxidation; and where the pipes containing gutta percha covered wires are filled with water, the decay of the percha is entirely prevented; but where the pipes are dry, and whenever the percha is exposed to air, it oxidises and perishes. The exterior becomes dry and brittle, and is easily cracked, the percha being changed into a resin, which is readily soluble in alcohol.

Lead and other substances have been used as a covering, but no real practical protection of gutta percha from oxidation has yet been accomplished. Mr. Physick and Mr. Fred. Webb employed a taping of zinc or some other soft metal for covering the gutta percha. My brother and I have extensively used tarred or painted tape, but the latter with only partial success; for the painted tape proved much more injurious to the percha than that saturated with vegetable tar. I conducted a very large number of experiments at the time of the Atlantic and Government Committee on submarine cables in 1859, with a view to discover some remedy, but, like everyone else, I failed; and one of the subjects most deserving the attention of this Society is that of endeavouring to find some practical means of preserving gutta percha from oxidation. When we remember how extensively it is employed, and what a scarce and costly material it is, every year becoming more scarce, it seems to me that this is one of the most important problems yet remaining unsolved.

Mr. TRUMAN: Some years ago Mr. Culley spoke in this room on the subject of joints, and I was bold enough to say that the joint could be made good. Time has shown that I was correct. Mr. L. Clark has just told us that the great desideratum is the durability of the gutta percha, and I think that I may say that also is accomplished—in fact, that can be secured. The durability depends upon matters which have not been thought of at all sufficiently. I hope to shortly be able to bring before the Society a few points which will considerably increase the durability of gutta percha.

Mr. C. F. FLEETWOOD: I agree with Mr. L. Clark, and think that the greatest enemy we have to contend with in London is oxidation. As regards joints being weak places, I am able to say that for the last three years ten faults have not arisen from bad joints in the streets of London. This fact rather contradicts statements made by many of our eminent members that joints are a source of trouble. The discontinuance of using Chatterton's compound has, in my opinion, reduced the number of faults, and I believe that when underground wires are completely buried, inherent faults will disappear. I recently recovered a short

portion of underground 12-wire cable (laid down, I believe, by Sir C. Bright), wrapped with a serving of tarred yarn, which cut as good as new; and, wherever underground wires are completely buried, percha lasts well. The nature of the ground affects gutta percha. A portion of 50 yards recently taken up near the York and Albany, and which passed over clay, was perfect, but when the portions of the same cable got into the gravelly soil, the percha was all broken to bits. Where the little insects referred to are found, the wires are generally smaller than when they were drawn in; but I do not think we are troubled so much with the insects as with boxes which freely admit the air.

Mr. ANDREW BELL supported the opinions expressed as to oxidation taking place with variations of temperature and access of air or moisture to the flush boxes. Within the last few years the method of protecting the wire had been changed. Tests of the old method of using tarred tape showed a loss of from 22 to 30 per cent. Paraffin was tried, but was found too brittle. Then ozokerit was applied, being much more plastic and pliable; and this method, instead of causing a loss, actually brought about a gain of from 10 to 12 per cent. when the wire is closely wrapped with the tape.

(In answer to Mr. L. Clark.) Ozokerit could not be applied to taped gutta percha wire in the same way as tar is applied—by running the wire through the tar—on account of its high melting point. The tape is first prepared by running it through melted ozokerit to which a small proportion of Stockholm pitch is added, to give substance and fill up the pores of the tape, and is served on the gutta percha wire when cold. Referring again to the use of Stockholm tar, it was necessary to heat it to about 100° Fah., in order to saturate the tape, and this degree of heat, together with the creosote in the tar, was no doubt the cause of insulation being so much impaired by this mode of protecting gutta percha wire.

Mr. A. J. S. ADAMS asked if atmosphere confined in sealed boxes was not liable to become hot or impregnated with gas, and so affect the gutta percha?

Mr. FLEETWOOD gav[illegible] ir in sealed boxes would become mo[illegible] ariations.

He had lately cured as many as 35 faults by cutting out a short piece of pipe and inserting a sealed box.

Professor AYRTON: I recently noticed in the *Electrotechnische Zeitschrift* that the plan followed in laying down the new underground wires in Vienna was to place them in troughing, and then fill up the troughing with cement, thus permanently closing up the wires in a solid mass, and carrying out practically on a large scale the sealed-box method.

Mr. L. CLARK: I may mention that it has now become the usual practice with the various submarine cable companies with which my firm is connected, to carry their connecting wires from the cable end to the testing house or office, where the land line ends in iron pipes filled with water; some sections of the kind being as much as 10 or 11 miles long. A reservoir of water is fixed at the highest level, and from this a constant supply is provided to the pipes, to replace any leakage that may take place, and keep them constantly filled with water. In hot climates this plan answers admirably; and although it has been very extensively employed during the past four or five years, not a single instance of fault has has occurred either from lightning, earth currents, insects, heat, or other causes. So far as experience has yet been gained, the plan is a perfect success, and it will in all probability become universally adopted.

Mr. D. BROOKS (Philadelphia), on being asked by the President to describe the system of laying wires underground, which had been adopted in Brussels and France, said: For twenty years I have tried to find some cheap system of telegraphy. Gutta percha, like every other vegetable compound, oxidises or decays when exposed to the air. Its preservation in water reminds me of the fact that in the swamps of New Jersey trees are now being dug up from a depth of 20 feet, which have been in the swamps for an unknown number of years, and are perfectly sound, and in fact have been preserved by their moist surroundings. So I think with percha: if it be kept constantly in *pure* water it is protected from decay. But gutta percha is expensive, and my object has been to substitute a cheaper material. I tried resin, pitch, and all kinds of hydro-carbons; tars, asphalts, and so forth, and buried

specimens in the ground; and, in the belief that I had something really valuable, applied for a patent, but found that somebody in England had travelled on the same road before me. My specimens began to fail. I filled the pipes with paraffin wax and worked the wires while submerged in water, but their insulation went down, due, I subsequently ascertained, to cracks in the paraffin wax admitting moisture. I conceived the idea of filling the pipe with an oil, and of using pressure in the pipe to keep out the intruding moisture, and that is the origin of my present system. I have here a specimen of the cable now being used with my system by the Western Union Telegraph Company between New York and Philadelphia: it is of 30 wires, each wound separately with jute, and 1,200 feet of this material are drawn in at a time.

The telephone working the wires are drawn into lead pipes, and I have here a specimen of telephone cable consisting of 84 wires It is found for telephone working that the smaller the wire the freer it is from induction and the familiar noisy rattle. This smallness of the wire admits of many being made up into a cable of small diameter, which can be easily run along the roofs and eaves of houses out of sight. That was about the latest novelty in America when I left on the 24th April last.

Mr. ANDREW BELL: What is the insulation of such wires?

Mr. BROOKS: It varies.* The insulation and electrostatic capacity is simply a question of distance between the wires and the thickness of covering. We have wires now that run to 1,000 or 1,500 megohms per mile, at a temperature of 70°, but if the insulation is very thin, then the figure falls or rises with varying temperature.

This system has led us to refine paraffin oil, to specially prepare it for the purpose; and when we get oil chemically pure, it stands, electrically, as high as glass or any other substance. With pure oil, good cotton fabric for separating the wires, and the absence of vegetable acids, the insulation is very high.

The PRESIDENT: What would be the insulation of wires such as the specimen you now hold?

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* See Abstracts, p. 324.

Mr. Brooks: The foreman of the Western Union Company gave it as 200 megohms, at a temperature of 100°. The greater the number of wires used in this manner for telephonic purposes the less the induction. When all the wires in a cable (as in the specimen before you) are connected with the earth, and two of them are used for telephone circuits, no interference is perceptible,—and in practice it seems that the effect is diminished in proportion to the number of wires,—and it is this fact that enables the wires (which are like spiders' nests) of the telephone exchanges in New York and Philadelphia to work without difficulty.

The President: I am very gratified to find that my few remarks have elicited so much interesting information this evening. The main fact that we have learned with regard to the durability of gutta percha is that certain conditions must be observed to procure durability. These are, to exclude from the percha the oxygen of the air, the variations of the temperature of the air, and the depredation of insects. All these qualities are contained in seas and rivers, and the plan described by Mr. Latimer Clark also fulfils them in the East and other places, and is worthy of trial in England.

Mr. Latimer Clark forgot to mention one little incident connected with this subject, viz., that many years ago I carried out an experiment, under his instructions, with the view of preserving decayed gutta percha wires. A lot of old cracked and decayed wires, running along a tunnel at Southampton, were taken and placed in wooden troughing, which was then filled with a mixture of tar and pitch, and the wires remained in perfect condition for 15 years, when they were disturbed by the removal of the wall. This is another proof that, with the exclusion of oxygen and animal life, gutta percha is very durable.

Before the next paper is read, I would say that we are much indebted to Mr. Brooks for the remarks he has favoured us with.

The following paper was then read:—

# ON A NEW ELECTRICAL SPEED INDICATOR FOR ENGINES.

By H. R. Kempe, Member.

The indication of the speed of engines has very often been attempted. Various instruments have been devised for the purpose, some complicated, and others simple in their character. Perhaps the simplest apparatus is one by Messrs. Elliott, fashioned on the same principle as the governor of a steam engine, the arms of the indicator diverging according to the speed of the engine. Another form consists of a glass tube rotated on its own axis in a vertical position. The tube is filled with oil, and the centrifugal force, varying according to the speed of the engine, causes the oil to rise or fall within the tube precisely at the rate at which the tube revolves. But all these indicators depend for their accuracy upon variable quantities, and only indicate at the place of motion. The electrical speed indicator now before you is able to do that which, as far as I know, none of its predecessors have been competent to do. It is specially adapted for correctly indicating the speed of an engine *working at a distance.* Besides, it possesses another peculiarity differing from other methods, inasmuch as it is not dependent upon variable quantities, but is timed by means of a clock. Clocks indicate divisions of time perfectly accurately, and therefore any instrument constructed with the action of a clock as its basis must give perfectly accurate results. This instrument before you indicates the speed of the engine every minute. I have the apparatus connected up to a small crank, which I turn to represent the motion of an engine. A clock is in circuit. On turning the handle a given number of times, a certain number of motions are produced in the instrument, and at the end of a minute, when the second-hand of the clock arrives at 60, the hand of the indicator will move to the number of times I have turned the wheel round, and will there remain until the next minute. If the speed at which the handle is turned varies, the indicator-hand will move backwards or forwards in sympathy. (Several illustrations were given.) I have not prepared diagrams of the instrument, and without them it is difficult to

explain its action, as the mechanism is somewhat peculiar. Each revolution of the engine wheel sends a current or pulsation to the indicator, and acts upon a ratchet wheel, which moves forward one tooth for each pulsation. At the end of the minute the hand connected but not fixed to the ratchet wheel is released, and moves through the same angle the latter has turned, and which represents the number of revolutions transmitted. As soon as the release has taken place, the indicator-hand is again locked, and the ratchet wheel returns to zero, ready to be again moved forward step by step. It is easy to understand how the needle can be made to follow up the wheel, but I think it is not so easy to understand how it can be made to move back, and it is this that is difficult to explain without diagrams.

Confusion might happen with such an instrument, by currents being sent by the engine at the moment the clock comes into action, but this is avoided by the use of an "accumulator." This accumulator receives and adds on, as it were, such interim currents, if I may so term them, to those to be recorded in the following minute. Of course the principal point in the instrument is, that it indicates the speed of an engine at a distance, so that all concerned may see with what regularity the engine is being worked. The necessity for such an instrument was felt at some waterworks, and it has been designed specially for that purpose; because at such places the engineman will often let the engine go slowly, and, finding the reservoirs going down, put on a spurt to bring the quantity to the proper mark, and these sudden changes rack the engines. By means of this indicator, a check is kept by the engineer in his office upon his subordinates. Of course, the instrument can be fitted to any engine.

Another use for such an indicator is in paying out telegraph cables, when it is necessary to know the number of revolutions per minute of the paying-out drum.

I shall be happy to answer any questions as to the mechanism, which is rather difficult to describe, as I said, without diagrams.

Mr. Latimer Clark: Of course, you could make it self-recording; say, for a day?

Mr. Kempe: Yes; all the indications could be easily recorded

by the instrument itself by a movable diagram less than two feet square, which is, I think, the minimum size on which such an instrument can be expected to record itself for twenty-four hours.

Mr. STROH: As I understand it, the instrument does not show variations during the minute?

Mr. KEMPE: That is so. It indicates the mean speed during the minute, which was thought sufficient for the large pumping engines for which it was designed.

Mr. STROH: I was thinking more of paying-out cables.

Mr. KEMPE: My experience in that respect is limited, but I should think that in the short space of a minute variations of speed could not be very great.

The PRESIDENT: Mr. Kempe forgot to say that he not only designed the instrument, but he actually made it himself. It altogether reflects very great credit on him, and I am sure we are all obliged to him for bringing it before us and so clearly describing it.

The following paper was then read:—

## ON AN IMPROVED FORM OF WHEATSTONE RECEIVER.

By J. WILLMOT, Associate.

The improved form of receiver which I have the honour of bringing before the Society this evening has been designed to meet the increasing requirements of the postal telegraph service, to secure a higher rate of speed.

As, no doubt, the majority of the members of this Society are aware, the present form of Wheatstone's Receiver is driven by means of a spring, which is somewhat irregular in its action, the speed of slip being 25 feet per minute when the spring is fully wound up, and only 19 feet per minute when the spring is near the end of its action.

In designing the improved form of receiver now before the Society, I have endeavoured to fulfil the following conditions:—

1. That it should record at fully 200 words per minute.

2. That the electrical and delicate mechanical portions should be easily detached from the more substantial parts of the instrument.

3. That it should be impossible to separate them without first locking the weight or driving portion.

4 That when the locking operation has been performed, it should be impossible to accidentally release it.

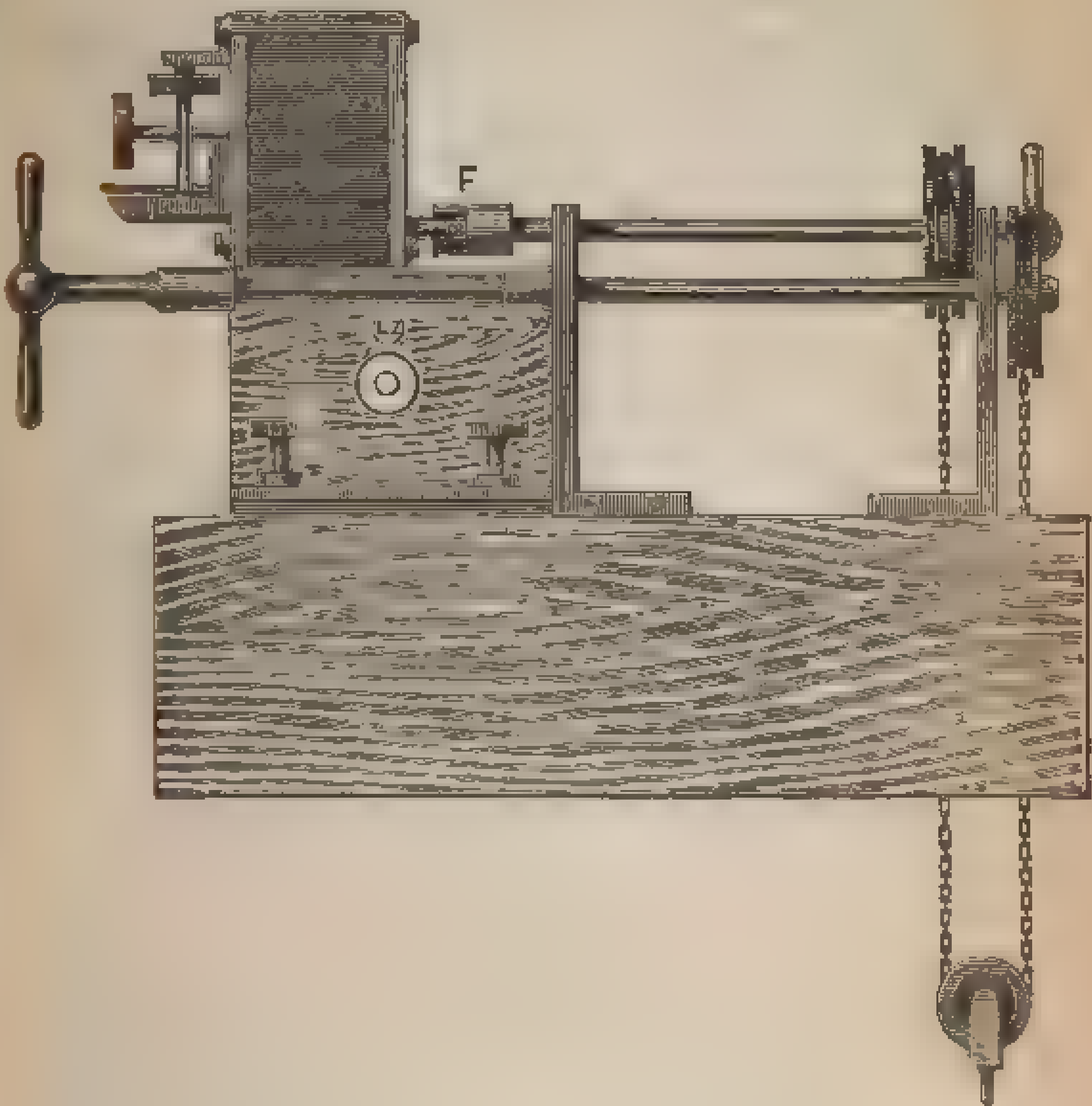
I will now ask you to be so good as to look at the diagram. You will observe that the two parts of the instrument are joined or geared together by means of a joint constructed upon that known as the bayonet principle, and that the action of the weight is to keep the flat surface of the joint pressing against the pin (A).


It will, therefore, be easily understood that the two portions of the instrument cannot be separated on account of the projecting part (B) of the socket preventing the withdrawal of the pin referred to.

You will also notice, by reference to the enlarged diagram (representing the locking gear), that the click wheel and "paul" are rather peculiar in shape, and that when the "paul" is placed in gear with the wheel it becomes locked, on account of the teeth of the wheel being undercut.


To separate one portion of the instrument from the other, it is necessary to raise the "paul" (E) until it passes its central position, the spring (C) then comes into operation and presses

the "paul" against the click wheel (D). By these means the weight is brought to a standstill, and is securely locked until the click wheel is turned (by means of the handle provided for the purpose) into such a position as to permit of the "paul" being easily disengaged.




When the weight has been brought to a state of rest (as already described), the pin (A) is carried forward, by reason of the momentum acquired by the trainwork, into the position shown upon the diagram at F. The parts can now be easily separated. The instrument now before the Society has already been worked upon a London and Liverpool circuit at a speed of 250 words per minute.

It is intended that that portion of the instrument comprising the paper draws, weight, etc., shall be permanently fixed in the operating rooms, and that the smaller portion, containing the more

intricate parts, should only be sent between the workshops in London and the various telegraph stations throughout the country.

In conclusion, I must state that the instrument, which will be shown in operation this evening, was manufactured by Mr. Stroh; and I think you will agree with me that it, if possible, surpasses his already high standard of mechanical work.

The meeting terminated with a ballot, at which the following were elected:—

*As Member:*

Dr. John N. Culbertson.

*As Associates:*

J. Mackenzie.

A. L. Paul.

The Ninetieth Ordinary General Meeting of the Society was held on Wednesday evening, May 26th, 1880, at the Institution of Civil Engineers, 25, Great George Street, Westminster—Mr. W. H. PREECE, President, in the Chair.

The minutes of the last meeting were read, and the names of new candidates announced.

The PRESIDENT: I have to call your attention to the fact that the catalogue of the Ronalds Library is now ready for distribution to subscribers. It has been issued in two forms: one a large handsome volume interleaved for remarks and additions, and the other a more portable volume. The prices originally fixed for the catalogue to subscribers were 10s. for the larger, and 6s. for the smaller copy; the prices to the public being 20s. and 16s. respectively. It has been decided by the Council to-day, that, to all members who have not already intimated their intention to subscribe, the price of the large volume will be 15s., and that of the smaller one 10s.; and it is requested that any one desirous of obtaining copies will hand in their names to the Librarian.

The following paper was then read:—

## A DECADE IN THE HISTORY OF ENGLISH TELEGRAPHY.

By EDWARD GRAVES.

When the idea of this paper was first suggested to me, I had intended to cover a much larger field than that I shall occupy by my remarks this evening.

It was my intention to have contrasted the position of the Electric Telegraph in 1869 and in 1879, as far as possible, throughout the whole world, including both submarine and land lines. But I found that the time required to obtain the necessary information, and the labour involved, would postpone the realisation of my idea until much too late; and I fear that, had I succeeded in carrying out my original design, I should have produced a mass of figures so unwieldy, that whatever interest they might have possessed in themselves would have been lost in their magnitude. Therefore, I have thought it preferable to

confine the remarks I propose to address to you to the facts of English Telegraphy proper, excluding entirely from consideration the submarine cables which connect our Islands with the Continents of Europe and America.

In popular estimates of the progress of telegraphic communication, it is usual to consider only the development of the electric agent as applied to the exchange of ordinary messages between individuals, and for the service of the Press—in other words, the commercial telegraph. It is sometimes overlooked that another sphere equally important is filled by it, and that it is to the growth and use of the electric telegraph that the multiplication of railway trains is made possible, and that their speed can safely be allowed to reach its maximum, however numerous they are. In fact, railway working, as we know it, would be impracticable were not the telegraph at hand to facilitate the necessary arrangements.

In reality, the use of the electric telegraph in this country was introduced in the first instance mainly, if not solely, for railway service.

In 1837, Cooke and Wheatstone's five-needle telegraph was tried successfully, on a small scale, by the London and Birmingham Railway Company near London.

Subsequently it was tried by the Great Western Railway Company between Slough and Paddington, and its use gradually but rapidly extended over most important trunk lines.

Wires were erected on the various railways by Messrs. Cooke and Wheatstone, the patentees of the only telegraph then in practical use. Their primary object was railway service—the exchange of messages between stations in the interests of railway traffic, but it was soon found that the public would employ the telegraph if it were thrown open to them, and the railway companies generally allowed their clerks to forward public messages, as a favour, upon payment of high rates. Thus we see that commercial telegraphy was, as it were, grafted upon railway telegraphy. The two grew together.

In 1846 the first telegraph company properly so called was

incorporated—the Electric Telegraph Company. The object of that company was with larger capital to continue the operations Messrs. Wheatstone and Cooke had undertaken, *i.e.*, to build telegraphs for railway purposes, being repaid the capital outlay by a rental charge or as might be arranged, and in either case undertaking to maintain and keep in order the telegraphs they put up. For this last service they received payment. An abatement, however, was made in their charges in consideration of the concession to them of certain rights. They obtained the power, for example, of putting up wires for themselves upon the same poles that supported those erected for the railway company; and they had the right of taking the receipts for messages from the public that might be earned by the secondary use of the railway wires as a further perquisite. In fact, in those days, although the relative growth of commercial telegraphy was considerable, seeing its recent commencement, still its absolute value was so far insignificant that the railway companies regarded it as a thing of little importance.

The Telegraph Company was also allowed to erect separate lines of poles and wires for itself on railways, and to avail itself of the services of the clerks of the latter in their spare time.

When the telegraphs were transferred to the State, one of the earliest duties devolving upon the Post Office was to disentangle itself from numerous agreements of the character indicated; and, unfortunately, this involved liberal compensation to the railway companies for all that they had lost, without any corresponding allowance for what the Department, succeeding to the telegraph companies, had surrendered.

As time went on, it was found that telegraph companies might subsist without the aid of railways. The public had become awake to the utility of their commercial operations, and the magnitude of the business was sufficiently considerable to render the speculators therein independent of the aid that had formerly been necessary for their support. Some time before the action of the Post Office brought on a complete separation of interests, there had been a growing tendency to divergence on the part of

railway and telegraph companies. Railway companies, in some instances, had determined to do the whole commercial business that could be earned at their stations on their own account, so far as they were enabled to convey it. Others had left the rights of the telegraph companies alone, so far as commercial matters were concerned, but had undertaken the maintenance and supervision of their own purely railway telegraphs. The time, in short, was visibly approaching when a community of interest would no longer exist; and had the telegraph companies carried on their operations until the existing agreements expired, there is no doubt but that, at best, they could only have renewed them on terms far less satisfactory to themselves, and far more profitable to the railway companies.

In one respect this separation of interests was unfortunate with regard to the all-roundness, so to speak, of the individuals employed in the carrying on and supervision of telegraphic operations.

Twenty years ago a telegraph engineer or superintendent had to interest himself, not only in the business of conveying commercial telegrams, and to consider the cheapest and most efficient means by which he could perform this service for the public, but he had also to study the needs and peculiarities of railway traffic. The same man, for example, was interested in the working of long circuits requiring high insulation and worked by rapid instruments, and in circuits to which insulation was comparatively of trivial importance, but which required for their operation apparatus capable of conveying, not words, as in the ordinary telegraph, but distinct signals, each of which should represent practically a sentence. The field in which such officers individually worked was larger than at present, but, of course, the actual work done was of less magnitude; and following the principle of the division of labour, possibly the separation has resulted in the more perfect development of both branches of the service.

I have therefore thought it right to treat the commercial and railway telegraphs of the kingdom as what they now actually are, distinct and definite things; and the first consideration is given to

THE PROGRESS OF COMMERCIAL TELEGRAPHY.

I have already stated that in 1846 the first telegraph company in England was incorporated.

A few years later, I think in 1850-51, the British Telegraph Company, the European Telegraph Company, and the Magnetic Telegraph Company all came into existence. After various vicissitudes, these companies, or what remained of them, amalgamated under the name of the British and Irish Magnetic Telegraph Company. This was in 1856-7.

Later still, in 1860 or 1861, the United Kingdom Telegraph Company, which had been projected many years before, assumed a real existence. This company differed from the former companies, inasmuch that it was wholly unconnected with railways. There were one or two short lines of railway over which its poles were erected; but I believe that in every case the railway was treated only as a route to be followed: there was no arrangement for mutual services or anything of that sort subsisting; at any rate, if there was, it was only the exception that proved the rule.

Then various local companies were established.

The London District Company, afterwards termed the London Provincial Company, had its sphere in the Metropolis and suburbs.

The Bonelli Telegraph Company, between Manchester and Liverpool.

The Economic Telegraph Company, and several others whose existence was ultimately unprofitable, and whose careers were brought to a more or less untimely end.

About 1861 another field of telegraphic operations was tapped on a considerable scale for the first time—the Universal Private Telegraph Company was started. Prior to that time, private wires, *i.e.*, telegraphs for the exclusively private service of certain individuals or associations, had been erected by the different companies existing; but this company commenced with the distinct object of laying itself out to construct wires solely for private use—from the offices of firms to their workshops, and the like. Afterwards it grafted a certain amount of public traffic in the West Highlands upon the original system, but to the end its main object continued the same, *i.e.*, the erection of private wires.

In 1869 there were existing in the United Kingdom no less than twenty-nine separate undertakings specially devoted to the transmission of telegraph messages, wholly independent of railway companies, who employed their wires secondarily for the same purpose. These undertakings varied in magnitude, from the Electric Telegraph Company, working throughout the whole of the United Kingdom, to the Tavistock and Princetown Company, which possessed one wire a very few miles in length, but all had to be acquired when the State was granted the monopoly of the transmission of messages for money; and, besides the land lines, it was necessary that the cables reaching to the Isle of Man and the Channel Islands should also be purchased.

There were certain railways which were carrying on telegraph business on a comparatively large scale. The districts occupied, for example, by the South Eastern Railway, London Chatham and Dover Railway, the London Brighton and South Coast Railway, and the North British Railway Company, had either never been wholly handed over to the telegraph companies, or had been taken out of their hands, so far as it was in the power of the railway companies to do so, prior to the date referred to, *i.e.*, 1869.

These railway companies were compensated for the actual business they were doing, and others obtained compensation in consideration of the loss of their reversionary power of carrying on such business, *i.e.*, when free from their engagements to telegraph companies.

In some instances a telegraph company and a railway company treated with each other as independent powers, but worked amicably. For example, the North British Railway Company and the Electric Telegraph Company were allied, but neither was annexed, so to speak, by the other. The North British Company possessed certain poles on its line, and, say, from Berwick to Edinburgh, the Electric Telegraph Company were entitled to have certain wires on those poles, and to work them from any point not on the railway, for their own benefit and at their own rates. On the other hand, the railway company picked up all the messages they could at their railway stations, and, if they were enabled to deliver them at another station, did so, retaining the receipts themselves.

If they were not able to carry a message to its destination, they handed it over, say, at Edinburgh, to the telegraph company, charging their own rate to that place, and the telegraph company's rate from it to the distant station. Thus it will be seen that in this case there were two distinct interests upon the same ground.

So matters were conducted until an agitation arose in favour of the control of the telegraphic communication of the country by the State. It culminated in a measure, passed in 1868, authorising the Post Office to buy up and extinguish existing interests in commercial telegraphy.

The Telegraph Act of 1868 did not contemplate granting a monopoly to the Post Office. Mr. Scudamore did not ask for it. However, before the Money Bill to make good the purchase was passed in 1869, the political authorities had determined that a monopoly of the right of sending telegrams for money was necessary, and it was therefore acquired.

The year 1869, the last complete year of the telegraph companies, was, in some sense, a period of stagnation. The arrangements made with the Government practically put an end to their future career in 1868, and until the purchase was completed their main object was to spend as little as possible, so as to realise the utmost immediate profits. The actual business grew, as it had acquired a habit of doing, and the receipts were larger than in the preceding year, but nothing was done to encourage the progress of telegraphy.

I propose, therefore, to take this period, when the active exertions of the companies were suspended, and compare its results with those attained in the year 1879, when for ten years the commercial telegraphs of the country had been transferred to the State.

In 1869 the lines of telegraph available for public use extended from Penzance to Wick, and from Lowestoft to Galway. In fact, the succeeding ten years have only carried them on to Scilly and the Shetlands, so far as regards the main north and south routes. But while the country was traversed by electric lines, it was not covered by them: they were very widely severed in some districts. Whole counties were dependent upon a single thread of communi-

cation, and towns far from a railway were in many instances utterly ignored. The telegraph had penetrated to places where commercial activity was rife, and whence profits could be drawn, but it had not been utilised for the benefit of rural districts, nor had it as a rule been established where commercial success could not be reckoned upon. In fact, the only chance of finding a telegraph office in a village was in the event of its also being a railway station where there was a telegraph available primarily for railway purposes, or of a trunk road line passing through to some large seat of commerce, upon which the company had established an intermediate office.

In 1879 the country was almost uniformly dotted over with telegraph offices. Commercial considerations alone were no longer kept in view, and telegraphic facilities were given in many instances to very small communities far from any commercial centre.

In 1869 the total number of telegraph offices was 2,488, railway stations open for telegraph business included.

In 1879 it had reached 5,331.

But the increase of convenience is in reality much greater than is expressed by these figures.

In 1869 there were three, and in some cases four companies all competing with each other, and all having offices in the same quarters of large towns, very often close together. So that, although a town might contain three telegraph offices, the accommodation to the inhabitants was really only that of one.

Under the present *régime* all this is changed; and to express the difference in the accommodation given, at least 500 offices should be added to the actual number now open, or rather a deduction of that number made from those formerly existing.

In 1869 the total number of messages transmitted by the companies was from 6,000,000 to 6,500,000.

I am obliged to estimate the number, because for this year no reliable statistics were kept. The figures of 1868 were compiled by most of the large companies, but the smaller ones were deficient in this respect for both years.

There is, besides, another source of inaccuracy which I do not

think has been observed in any former comparison of the kind, that is, each company counted the number of messages passing over its own wires without regard to the fact that the same messages were counted again on the wires of another company. Let me illustrate. A message proceeding from Douglas to Galway is of course in 1879 one message. But a similar message in 1869 would be reckoned one message on the Isle of Man Company's line, one message on the Electric Company's line, *i.e.*, from Whitehaven to Dublin, and one message on the Magnetic Company's line, from Dublin to Galway. The total number of messages thus transferred from one company to another was very large, and in consequence the published figures on the point are vitiated by a great inaccuracy. I believe, however, that the totals I have given are at any rate a close approximation to the fact.

The total number of messages transmitted over the English land lines in 1879 (more exactly for the financial year ending 31st March, 1880) was 26,547,137, being an increase of more than fourfold over those of the corresponding period ten years ago.

In 1869 the total number of miles of wire used exclusively for commercial traffic was under 50,000, some part of which length was by the provisions of the Acts of 1868 transferred to railway companies upon the acquisition of the telegraphs by the State, and does not now count as commercial wire.

In addition, the telegraph companies had the partial or secondary use of the railway wires.

| | | |
|---|---|---|
| In 1879 the mileage of Post Office wires used for commercial traffic, existing on railways was 54,067 miles. | | |
| Existing on roads and canals ... | 46,784 | ,, |
| Existing in the local submarine cables ... ... ... ... | 1,805 | ,, |
| | 102,656 | |

This mileage also was supplemented to some extent by the use of railway wires, which, under various arrangements, are still available to the public.

In 1869 the number of instruments fixed upon commercial

wires, and used to work them, was as nearly as can be ascertained about 2,200.

In 1879 the number was, of various kinds, 8,151.

On this point I may mention that the increase in the rapidity of the instruments employed, *i.e.*, their average power of transmitting messages, is far greater than that of their numbers.

In 1869 there were, for example, two Wheatstone automatic circuits existing in Great Britain,—one London to Manchester, one London to Newcastle,—worked by four instruments.

In 1879 there were 173 such instruments employed.

In 1869 no duplex instruments existed.

In 1879 there were 392 employed.

This means, in effect, that wires formerly worked by ordinary instruments had been practically doubled in numbers by the adoption of the duplex system.

In 1869 quadruplex of course had not been thought of.

In 1879 there were six circuits worked by that invention, making the six wires equal to twenty-four in carrying capacity.

In 1869 no Morse sounder instruments were employed.

In 1879 there were 1,374 of these instruments in use, the result being that an equal number of Morse printers were dispensed with.

Various statements have been made from time to time to the effect that the Government monopoly of commercial telegraphs stifled invention. Such statements are wholly without foundation. No invention promising either superior results to those previously attained, or promising to effect equal results with greater economy, or other distinct advantages, will ever be otherwise than welcome to the Department. Whatever will enable it to do its work better or cheaper must surely be as valuable to the State as it could be to any private company.

I was looking the other day at the report of the examination of Mr. Scudamore on the 1st July, 1868, before the Parliamentary Committee, when he was asked whether the Government would be less or more likely to take up improvements than the Companies would. He replied: "On the whole, I think that the Government would be more likely to take up new improvements. In one

respect we should be nearly on the same footing as the telegraph companies have been with regard to improvements. When a telegraph company has had a large amount of capital locked up in any particular kind of instruments, it would naturally hesitate to employ another. This though it might have great superiority over the existing one,—and the Government to a certain extent would have the same disinclination."

This answer expresses the truth of the case. Where an alteration would have the effect of throwing out of use a large stock of apparatus, for example, in which a very large capital had been embarked, it is clear that such alteration must be of very decided advantage,—a trifling gain over existing means would not be sufficient—to justify the commercial waste, so to speak, that must arise from its adoption. But if there was a great advantage, even this loss would be submitted to, rather than that the invention should be ignored. No doubt, in one respect, inventors do not find the market quite so open to them as formerly. When it was possible for several commercial companies to struggle against each other, it was always possible for the inventor of a novelty (although such novelty might be in no essential particular really superior to what was previously existing) to get the assistance of a promoter to form a company to work it. Thus, when the Electric Telegraph Company were depending chiefly on Cooke and Wheatstone's needle instrument, one of their first rivals was started to work Henley's magnetic needle telegraph, although most certainly, so far as the public were concerned, the working of this apparatus gave them no advantage, whatever the creation of an opponent company might do. In fact, not long after, the company owning it employed other instruments.

In this respect, no doubt, the Monopoly Act has closed one field of speculation; but the Post Office cannot be blamed for an unavoidable effect, and perhaps not altogether an undesirable one.

But wherever good has been found it has been availed of. The inventive genius of our transatlantic cousins has had a fair field of action in our borders, and its worth has been fully and practically acknowledged.

As an illustration of the steady, though not showy progress

that the Post Office has made in the improvement of its apparatus, —a degree of improvement really far more valuable than the introduction of ordinary inventions,—I may state that, when Sir Charles Wheatstone brought his automatic instrument to the Electric Telegraph Company in 1866, it was only possible to work it at the rate of about 55 to 60 words a minute if a wire of No. 8 gauge was employed, increasing to 80 words a minute if a wire of No. 4 gauge was used: this over a circuit about 280 miles in length.

In 1879 it was possible to work for distances of 300 miles with No. 8 gauge wire at over 200 words a minute, and regularly and in all weathers at 130 words.

Thus the Wheatstone of 1879 was really more than twice as efficient as the Wheatstone of 1869, and this improvement has been brought about almost wholly by the officers of the Department and of the former Telegraph Company, and by the skill of the mechanics acting with them.

In 1869, to insert a relay upon a wire was to deliver stronger signals at the far end when the lines were in bad order, and thus to make working for long distances possible when otherwise it would have been impossible; but from mechanical defects the insertion of relays invariably retarded speed in good weather.

In 1879 relays inserted on a long-distance wire increased its speed positively in the finest weather, because, of course, they practically shorten the circuit, while the improvement of the mechanism is such that in themselves their mechanical operation is productive of no retardation whatever. Thus, wires between London and Dublin are now habitually worked with relays inserted at a place near Bangor, at a speed reaching 130 words a minute, although a cable fully 70 miles in length forms a portion of the circuit.

Continuing the details of the available means of communication, I may mention that in 1869 the length of internal submarine cables, *i.e.*, of those connecting England and Ireland and the various outlying islands, wholly independent of those uniting this country to the Continent, was about 121 knots, the total being made up of 18 separate cables.

incorporated—the Electric Telegraph Company. The object of that company was with larger capital to continue the operations Messrs. Wheatstone and Cooke had undertaken, *i.e.*, to build telegraphs for railway purposes, being repaid the capital outlay by a rental charge or as might be arranged, and in either case undertaking to maintain and keep in order the telegraphs they put up. For this last service they received payment. An abatement, however, was made in their charges in consideration of the concession to them of certain rights. They obtained the power, for example, of putting up wires for themselves upon the same poles that supported those erected for the railway company; and they had the right of taking the receipts for messages from the public that might be earned by the secondary use of the railway wires as a further perquisite. In fact, in those days, although the relative growth of commercial telegraphy was considerable, seeing its recent commencement, still its absolute value was so far insignificant that the railway companies regarded it as a thing of little importance.

The Telegraph Company was also allowed to erect separate lines of poles and wires for itself on railways, and to avail itself of the services of the clerks of the latter in their spare time.

When the telegraphs were transferred to the State, one of the earliest duties devolving upon the Post Office was to disentangle itself from numerous agreements of the character indicated; and, unfortunately, this involved liberal compensation to the railway companies for all that they had lost, without any corresponding allowance for what the Department, succeeding to the telegraph companies, had surrendered.

As time went on, it was found that telegraph companies might subsist without the aid of railways. The public had become awake to the utility of their commer[illegible] operations, and the magnitude of the busine[illegible] sufficie[illegible]iderable to render the speculators therein [illegible] had formerly been necessary for th[illegible] the action of the Post Off[illegible]terests, there had [illegible] part of

to the season, are despatched as special reports for various London and Provincial newspapers, from their own correspondents, being wholly additional to the general news supply.

There are certain newspapers to whose use special wires are devoted, *i.e.*, instead of receiving intelligence transmitted over the ordinary wires in the ordinary way, they have arranged to have certain wires given up for their sole use from 6.0 p.m. to 6.0 a.m., or other hour, and send over them whatever matter they like without its being counted as business done by the Post Office, clerks being lent to them for the purpose.

The Electric Telegraph Company, prior to the transfer, had inaugurated this system.

In 1869 there were a total of 6 special renters, each paying from £750 to £1,000 per annum for the wire appropriated to them. In 1879 there were 24 such renters, each paying £500.

In the figures previously stated with regard to the mileage of wire and number of instruments employed for commercial purposes in the two contrasted periods, I have excluded from notice those referring to private wires, except such as were constructed by the Electric Telegraph Company and the Magnetic Telegraph Company. They were of no great extent, but to whatever extent it was, I am unable to disentangle the "private" wires from the "public" wires. This causes a slight error, inasmuch as the following figures with reference to the private wire system contain for 1869 some portion of the same mileage as has been already calculated for the commercial telegraphs of the same year.

On the whole, the private telegraphs (chiefly belonging to the Universal Private Company, but in a smaller degree to other companies) amounted to 2,525 miles of wire, worked by 1,466 instruments, in 1869.

On the 31st December, 1879, the Post Office maintained for the use of private persons 6,550 miles of wire and 3,824 instruments, or, including wires used by railway companies in Ireland for their own purposes, and by certain canal companies, maintained also by the Post Office staff, the total was 9,269 miles of wires and 4,607 instruments.

It should be remembered that the Post Office possesses no

monopoly of private wires, and that, in calculating the whole existing telegraphs of the country, a large addition must be made to the figures before stated to represent the wires and instruments fixed in various localities by private contractors, not forgetting the recent active operations of the various telephone companies.

A fair comparison between the earnings of the English telegraphs in the two periods under review is difficult to make.

In 1869 the Electric Telegraph Company, for example, reckoned among its earnings business now considered as belonging to the Submarine Company, *i.e.*, earned by means of cables connecting England with the Continent.

The position of all the land telegraph companies towards the respective cable companies with which they were connected differed to a more or less degree from that of the Post Office and the cable companies at the present time.

The receipts from telegraphs earned by the various railway companies in 1869 are in some instances inextricably mingled in their accounts—they cannot be accurately stated.

The great alteration in rates (especially in favour of the Press) that has taken place, and the fact that many of the extensions during the last ten years were certainly not undertaken on commercial principles, destroy all identity between the two periods in this respect. Hence we have figures which are hardly to be compared. So far as it can be ascertained, the gross earnings of all English telegraphs in 1869 may be stated with certainty as not more than £700,000.

In 1879 the earnings of the Post Office Telegraphs were on the whole £1,436,000—a sum more than double the amount of the largest earnings of the telegraph companies. Moreover, ten years after the transfer of the lines to the State had been effected, the balance-sheet of the Department shows that the telegraphs have become practically remunerative, *i.e.*, that for the financial year ending 31st March, 1880, there is for the first time almost an equilibrium between receipts and expenditure, including in expenditure the full interest on the capital borrowed for the purpose of purchasing and extending the telegraph undertakings, and the total capital cost of all extensions during the year for which

the receipts are stated; from which latter are excluded the value of services performed for other Government Departments, and value of materials, &c., sold, amounting on the whole to £36,000. To be exact, in the year 1878-79 the sum required from the national coffers to make up the actual interest on capital was over £114,000; for the year 1879-80 the precise calculations are not made; but although the amount of capital on which interest has to be paid is increased by a sum absorbing over £4,000 per annum, the deficiency will pretty certainly be not above £30,000. It is probable that, if the accounts were compiled on the system that would be adopted by a commercial company, this deficiency would be converted into a surplus of a nearly equivalent sum.

Before quitting this branch of my subject, it may be of interest to observe that in 1869 the working of the various telegraph undertakings, other than those of railway companies, employed a total force of 2,514 clerks, (479 of whom were females), and 1,471 messengers.

In 1879 the clerks numbered 5,611, (of whom 1,556 were females), and 4,648 messengers.

It should be observed that only professional clerks, so to speak, whose whole time is or was devoted to telegraphic work, are enumerated in these comparisons. Railway booking clerks, on the other hand, and assistants to postmasters, a very numerous body, who are not solely occupied by their telegraphic duties, are not reckoned.

Comparisons are often made between the development of the postal service and that of the telegraphic service. In such comparisons there are various elements of vital difference that are often wholly overlooked, but yet the absolute results may be fairly stated. For the financial year ending 31st March, 1873, the number of telegrams forwarded throughout the United Kingdom was 15,535,000; it was for the year ending 31st March, 1880, over 26,500,000, being an addition of nearly three-fourths in seven years.

The first year taken for this comparison is the one I have named (1873), because otherwise due allowance would not be made for the immediate expansion of business which arose after January,

1870, in consequence of the large increase in the number of offices, and of the operation of the uniform shilling rate.

By March, 1873, the public had become accustomed to the new state of things. No radical change in the commercial conditions of telegraphy has since taken place, although, of course, continual provision of increased accommodation and the like has been going on to meet increased work.

For the year ending March 31st, 1873, the number of letters and post-cards was 976,000,000; and for the year ending March 31st, 1880, about 1,242,000,000. Thus, while in the period the use of the telegraph has increased by three-fourths, that of the post has not increased by one-third. This result is in one sense a matter of course. The older and full-grown service could not develop at the same ratio as the younger one, but the fact at least proves that the constitution of the latter is sound, and gives promise of continued vitality.

Still there is always this broad distinction to be kept in view: the post is an agency employed at some time or other by every inhabitant of the empire who is able to read and write, and indeed by many whose letters have to be written for them, and the result is that the number of letters and post-cards approaches very closely to 37 per head of the entire population of our islands; whereas, on the contrary, speaking roughly, it requires five inhabitants of the United Kingdom to send four telegrams; and although no doubt there is scope for further abundant increase, still it is pretty evident that under no conceivable conditions of tariff can the employment of the telegraph ever rival in extent that of the post. Nevertheless, it is satisfactory to find that, as things stand, the use of the telegraph is greater in England than in any European country of magnitude—that in this, as in other business matters, we keep well in advance of our neighbours.

Next to England, in order of telegraphic importance among the countries of Europe, come France and Germany.

There has not yet been time to obtain the foreign statistics for the year 1879, and therefore I will use those of 1878, contrasting them, of course, with the English figures for the same year.

In that year the population of Great Britain and Ireland was

reckoned at 33,799,000, and this population exchanged a total of 24,600,000 messages.

In France the population of 36,900,000 (an excess of more than 3,000,000 over that of Great Britain) employed the telegraph to the number of 14,400,000 messages, or upwards of 10,000,000 less than the number passed over the wires of this country.

Germany contrasts even more unfavourably.

With a population of 42,700,000, the number of telegraphic despatches was barely 100,000 more than those of France, or 14,540,000.

The only cases in which the proportionate number of messages is at all larger than in Great Britain are those of Belgium and Switzerland.

In neither of these States does the total number of messages amount to 1 per head of the population, and in Belgium the number forwarded is even closer to the British ratio as compared with population. But both these countries derive an exceptional advantage from the number of foreign telegrams passing through them, and not, as it were, resting within their borders. English messages are not swollen to any appreciable extent from this cause. Switzerland, moreover, has its native business largely increased by the host of locomotive and telegraphing foreigners annually attracted to the playground of Europe. Hence I think it may be fairly said that England is decidedly ahead of Continental Europe in the matter of telegraphic employment.

Thus far has commercial telegraphy advanced in this country since it has been affiliated to the Post Office.

During the present year arrangements have been made for the erection of over 5,000 miles of new wire, intended to meet the demands upon the system made by the prospect of reviving trade. The cost will not reckoned as capital, but will be all charged to the revenue of the Department.

For two or three years past, owing to the commercial stagnation that has prevailed, the rate at which the use of the telegraph has progressed has been but slight. Indeed, for the first six months of the last financial year, the business was absolutely stationary. Since that date the increase of business has been at a

varying ratio of from 10 per cent. to 22 per cent. per week. It is to be hoped that prosperity will continue, that the growth of telegraphic traffic will continue, and that the administration of the telegraph will be able to cope satisfactorily and creditably with the business entrusted to it.

I am an officer of the Post Office Telegraphs. It is not my business, at any rate in public, to speculate upon the future of the service. It must be recollected that those who are responsible for the conduct of the telegraphic administration have had in the past one prime duty to perform—one chief object was authoritatively pressed upon them—to make it pay. To do the work efficiently, but to risk no revenue. To make an agent advantageous, and very advantageous, to a section of the population—no longer a burden upon the whole. That result may be considered as achieved, or very nearly so. Until it was achieved, the time for experiments in any direction that could jeopardise the revenue had not arrived. It is for those who direct the financial policy of the nation to indicate the future course to be pursued. The permanent servants of the State and of the public have but to conduct the affairs of the Department entrusted to them in such wise as shall best carry out the views of those whose instructions must guide them, and to contribute, as far as in them lies, to whatever will tend most to the public advantage.

## RAILWAY TELEGRAPHY.

I now enter upon the second branch of my subject, and fortunately I can deal with it somewhat briefly. It does not ramify into so many branches as does commercial telegraphy; and the great progress it has made is more strikingly expressed by the statistical table that will be found printed at the end of this paper, than by anything that can be described in mere words.

In 1869 the "block" system, although it was a thriving child, was comparatively in its infancy, and the adoption of telegraphy to any large extent, as a means of indicating the correct operation or otherwise of distant fixed signals, was confined to a very few companies.

As is well known, the block system resulted from the experience gradually obtained by railway companies as to the impossibility of avoiding collisions so long as they relied upon an interval of time only to separate their trains. It was found that an interval of distance must be provided; and it is to the electric telegraph that the possibility of doing this is due.

On any length of railway it is only necessary to divide the line into short sections, and to control each of them by an electric telegraph, on the simple principle that a train should not be allowed to enter on any one of the sections until the train preceding it had been signalled from the further end thereof as having completed its run to that point.

After the soundness of the principle had been proved by the test of actual use, it was gradually adopted on some of the most important lines; and I find, for example, that the London and North Western Railway Company had, in 1869, 511 block instruments (*i.e.*, instruments a combination of signal-needle and bell) used for thus dividing the railway.

But in 1879 it will be seen that the number of instruments of similar character had grown to upwards of 3,000. To a considerable degree this great increase (sixfold) arose from the extension of the block system to new branches or lines of railway, but it was also to a great extent due to the subdivision of the sections originally arranged; as, for example, would be the case when a line originally divided into lengths averaging two miles each was divided into lengths of one mile.

Signal repeaters also show a very marked increase on almost all railways. Their use is to notify to the man putting on a distant signal that the semaphore arm really points in the manner intended by him; or that, when signal lamps have been lighted some distance away from the station, they are burning properly, and indicating that which they are intended to do. Should the semaphore arm move from the position in which it has been placed by the levers, an indicator in the signalman's box whence the levers are worked will immediately point and show that something has happened, or a bell will ring. In the same way, another indicator arranged in an electric circuit, to show when a lamp is

burning steadily, will shift its position and alter its direction if the lamp goes out.

There are infinite varieties both of the block and repeater telegraphs, but the object and main principle of all is the same.

The utility of these arrangements, as securing the continuity of perfect signals, is self-evident, as in many instances both semaphores and signal lamps are only imperfectly, if at all, visible from the stations or junctions they are intended to protect.

It will be seen that almost all important railways have very much enlarged their employment of this useful branch of electric agency. The North Western Company, for example, have increased its employment from 10 to 1,715 signal indicators, and the Great Western from 410 to 1,865 during the ten years.

I have to express my thanks to the managers, engineers, telegraph superintendents, and other officers of the various railway companies to whom I am indebted for the table of statistics relative to railway telegraphs that I have been enabled to compile. I am not sure that the figures have been returned to me in every instance upon the same principle, and there may, for example, be an occasional confusion as to the functions of a signal bell: sometimes it may have been reckoned as part of a block apparatus, to which it is really an auxiliary; at other times it may have been regarded as a distinct signal instrument, which sometimes it really is, being employed without a block instrument. But subject to points of this nature, I have little doubt that the figures can be relied on, and that they form a comprehensive statement of the facts of railway telegraphy for the year 1879.

It will be seen that I have totalled the figures for 1879, because I have them from every railway; but those for 1869 are in some instances irrecoverable, and in other instances imperfect, owing to various causes. Thus there is no total to this side of the statement; but ample materials for comparison exist between the separate items.

It would be tedious to dissect the table, which every reader can do at his own leisure, but it is noteworthy that the largest relative increase in the employment of the telegraph is on the Festiniog Railway, where in 1869 half-a-mile of poles, carrying

half-a-mile of wire with a block instrument at each end, existed, and where now there are 13 miles of poles, a twenty-sixfold increase, with 29 miles of wire, a fifty-eightfold increase.

The largest absolute increases are on the London and North Western, where 1,086 miles of poles have been increased to 1,840, an addition of 756 miles, and the mileage of wire has increased from 3,513 to 7,684, an addition of 4,173; and on the Midland, where the mileage of poles (including the Hereford, Hay, and Brecon) has increased from 783 to 1,244, being an addition of 457 miles, and that of wire from 3,248 miles to 8,082 miles, an increase of 4,834 miles.

On the whole, there were 62,100 miles of wire devoted to railway use, with 13,128 speaking instruments, and 33,719 block and other distinct signals of all kinds, on the 31st December, 1879.

To this extent is the telegraph an auxiliary to railway service.

Viewing the progress made in the development of English telegraphy as a whole during the past ten years, we who have taken part therein, however humble, may well be proud of it. In every field and every branch a great onward stride has been made. Let us trust that the next ten years will show as satisfactory a record. Our motto must ever be "Onward." The attainments of the past are but an earnest of what the future should and will achieve.

| RAILWAY. | Year ending 31st December, 1869. | | | | | | Year ending 31st December, 1879. | | | | | |
|---|---|---|---|---|---|---|---|---|---|---|---|---|
| | Mileage of Poles. | Mileage of Wire. | Number of Instruments. | | | Remarks. | Mileage of Poles. | Mileage of Wire. | Number of Instruments. | | | Remarks. |
| | M. | M. | Speaking. | Block. | Repeaters and Special Signals. | | M. | M. | Speaking. | Block. | Repeaters and Special Signals. | |
| Brecon and Merthyr ... ... .. | 67 | 119 | 49 | 2 | .. | | 61 | 147 | 30 | 43 | 2 | |
| Caledonian ... .. ... ... ... | 398 | 1,120 | 196 | .. | 45 | | 961 | 3,252 | 1,020 | 920 | 362 | |
| Cambrian .. .. ... ... .. ... | 85 | 170 | 25 | 4 | . | | 173 | 426 | 66 | 88 | 2 | |
| Carmarthen and Cardigan . .. | 17 | 34 | 16 | . | ... | | 19 | 38 | 7 | 11 | ... | |
| Cheshire Lines Committee ... ... | 60 | 158 | 23 | 20 | 19 | | 105 | 543 | 117 | 330 | 4 | |
| Cornwall (South Devon, Cornwall, West Cornwall, and Cornwall Mineral ... ... | 201 | 531½ | 90 | 169 | 2 | | 327 | 759 | 154 | 206 | 117 | Now part of Great Western Line. |
| Festiniog ... ... ... ... ... ... | ½ | ½ | ... | 2 | ... | | 13½ | 29¼ | 12 | 6 | 2 | |
| Furness ... ... ... ... ... ... | 93 | 199 | 50 | 22 | ... | | 100¼ | 320¾ | 83 | 122 | 12 | |
| Glasgow, Barrhead, Kilmarnock Joint ... ... .. .. .. | 6¾ | 13½ | 2 | ... | ... | | 29 | 103 | 30 | 36 | 23 | |
| Glasgow and Paisley Joint .. ... | ... | 20 | 2 | 18 | 2 | | 1¼ | 33 | 21 | 48 | 54 | |
| Glasgow and South Western .. | 234 | 845 | 78 | 8 | 2 | | 325 | 1,062 | 258 | 236 | 54 | |
| " " " City of Glasgow Union ... ... | ... | ... | ... | ... | ... | | 6 | 13 | 3 | 23 | 8 | |
| Great Eastern (including Tilbury and Southend) ... ... | 798¼ | 2,666½ | 310 | 122 | 70 | | 1,062 | 3,624½ | 655 | 643 | 432 | |
| Colne Valley .. ... ... ... .. | 19 | 38 | 7 | ... | ... | | * | 38 | 7 | ... | ... | *Poles banged to Post Office in 1879. |
| Great Northern ... ... ... .. | 534¼ | 1,574 | 165 | 446 | 2 | | 720 | 4,190 | 1,074 | 2,548 | 160 | |
| Great North of Scotland ... ... | 283¼ | 719¼ | 78 | 56 | 5 | | 283¾ | 700½ | 119 | 92 | . . | |
| Great Western (proper) ... .. | 346 | 948 | 261 | 503 | 410 | | 1,549 | 6,428 | 1,887 | 1,807 | 1,865 | |
| " " Bristol and Exeter | 132 | 476 | 68 | 98 | 15 | | 217 | 671 | 144 | 172 | 63 | |
| Greenock and Wemyss Bay ... .. | 10 | 20 | 3 | . | .. | | 10 | 22 | 8 | 12 | 4 | |
| Highland .. .. ... ... ... .. | 258¼ | 701½ | 75 | 43 | . | | 400¼ | 1,236¼ | 147 | 69 | 5 | |
| Lancashire and Yorkshire .. ... | 422 | 1,063 | 819 | 65 | 122 | | 582 | 3,031 | 491 | 1,091 | 915 | |
| London and North Western... .. | 1,086¼ | 3,513 | 626 | 511 | 10 | | 1,840 | 7,684 | 1,641 | 3,015 | 1,715 | |
| London, Brighton, & South Coast | 79¾ | 193½ | 46 | ... | 1 | | 381½ | 1,698 | 609 | 572 | 339 | |
| London, Chatham, and Dover .. | 141½ | 659¼ | 169 | 367 | .. | | 158 | 738 | 222 | 414 | 414† | † Including 249 "Electric Locking Signals." |
| London South Western... .. . | 659 | 2,010 | 239 | 251 | 44 | | 774 | 3,670 | 515 | 727 | 835 | |

STATISTICS OF RAILWAY TELEGRAPHS FOR THE DECADE, 31ST DECEMBER, 1869, TO 31ST DECEMBER, 1879—*continued.*

| RAILWAY. | Year ending 31st December, 1869. | | | | | | Year ending 31st December, 1879. | | | | | |
|---|---|---|---|---|---|---|---|---|---|---|---|---|
| | Mileage of Poles. | Mileage of Wire. | Number of Instruments. | | | Remarks. | Mileage of Poles. | Mileage of Wire. | Number of Instruments. | | | Remarks. |
| | | | Speaking. | Block. | Repeaters and Special Signals | | | | Speaking. | Block. | Repeaters and Special Signals | |
| | M. | M. | | | | | M. | M. | | | | |
| London & So. Western—Somerset and Dorset | 65 | 192¼ | 29 | 44 | ... | | 91½ | 514 | 54 | 72 | 25 | |
| " " " West London Extension | 4½ | 24 | 18 | 26 | 6 | | 4½ | 29 | 22 | 26 | 9 | |
| Macclesfield, Bollington, and Marple ... ... ... ... ... | ... | ... | ... | ... | ... | | 11 | 44 | 9 | 28 | ... | |
| Maenclochog ... ... ... ... ... ... | ... | ... | ... | ... | ... | | 8 | 24 | 5 | 6 | ... | |
| Manchester, Sheffield, and Lincoln | 265 | 489 | 103 | ... | ... | | 296 | 1,173 | 367 | 506 | 76 | |
| Manchester South Junction and Altrincham ... ... ... ... | 9 | 43 | 10 | 48 | 24 | | 19 | 90 | 19 | 78 | 32 | |
| Maryport and Carlisle ... ... ... | 28 | 56 | 5 | ... | ... | | 28 | 58 | 13 | 8 | 2 | |
| Metropolitan ... ... ... ... ... ... | 10 | 73½ | 121 | 160 | 91 | | 24 | 201 | 214 | 287 | 204 | |
| Midland ... ... ... ... ... ... | 1,017 | 3,248 | 306 | 294 | 374 | | 1,244 | 7,871 | 1,191 | 3,590 | 762 | |
| Mid Wales ... ... ... ... ... ... | 48 | 96 | 13 | 18 | ... | | 48 | 96 | 19 | 24 | ... | |
| Neath and Brecon ... ... ... ... | (Line worked by Staff and Tickets.) | | | | | | 11½ | 11½ | 5 | 4 | ... | |
| North British ... ... ... ... ... | 655 | 1,366 | 200 | 87 | 13 | | 765 | 2,384 | 409 | 411 | 88 | |
| North Eastern ... ... ... ... | *804½ | 2,193¾ | 262 | 6 | 178 | * Not including Stockton and Darlington, figures irrecoverable. | 1,548¼ | 6,614 | 923 | 2,892 | 2,435† | †Probably partly auxiliary bells used in connection with block apparatus. |
| North Staffordshire ... ... ... | 141¾ | 343½ | 83 | 206 | ... | | 166 | 431 | 98 | 256 | 70 | |
| Oldham, Ashton-under-Lyne, and Guide Bridge Junction | 5¼ | 11 | 3 | ... | ... | | 5¾ | 31 | 17 | 44 | 8 | |
| Pembroke and Tenby ... ... ... | 27½ | 43 | 8 | ... | ... | | 27½ | 55 | 10 | 12 | ... | |
| Rhymney ... ... ... ... ... ... | 29 | 107 | 18 | 27 | 7 | | 29 | 107 | 18 | 27 | 7 | |
| Severn and Wye and Severn Bridge | | (Tramway in 1869.) | | | | | 25 | 53 | 22 | 28 | 29 | |
| South Eastern ... ... ... ... ... | 335 | 1,494 | 277 | 626 | ... | | 340 | 1,618 | 360 | 851 | 147 | |
| South Wales Mineral ... ... ... | 12 | 13½ | 3 | ... | 3 | | 12 | 13 | 4 | ... | 3 | |
| " " Governor & Co. of Copper Miners | 3 | 3 | 2 | 2 | ... | | 3 | 3 | 2 | 2 | ... | |
| Taff Vale ... ... ... ... ... ... | 50 | 115 | 45 | 2 | ... | | 70 | 189 | 82 | 84 | 29 | |
| Whitland and Taff ... ... ... ... | | (Not open.) | | | | | 14½ | 33 | 5 | 4 | ... | |
| | | | | | | Total ... | 14889¼ | 62099½ | 13,128 | 22,411 | 11,808 | |

The PRESIDENT: I do not know whether this paper can be considered as one which should call forth a discussion, but I am quite sure that Mr. Graves is ready to answer any questions that may be asked, and we shall be very happy to hear the observations of any member.

Sir CHARLES BRIGHT: I have no criticism to make on the paper, which is a record of facts that will commend itself to all interested in the progress of telegraphy. It is a very creditable bill of fare, and the details for the years subsequent to the taking over of the telegraphs by the State could not be reliably supplied by any but a principal officer of the Postal Telegraphs.

Many difficulties had to be encountered by the Government in taking over the telegraphs in 1870. There were several distinct systems, with their respective organisations, to be merged into one machine. I can speak of these difficulties of my own knowledge, and know I shall be supported by Mr. Latimer Clark, who was the engineer of the Electric Telegraph Company, while I was in a similar position as regarded the smaller but not inconsiderable concern belonging to the Magnetic Telegraph Company. Besides staff difficulties there was also that of dealing with two codes or alphabets, one of which had to be abolished, and the staff generally trained to the other, and all these questions were dealt with, we know how ably, by the officers of the Post Office, most of whom had long experience in the employ of the old Companies. I know that you are all familiar with this, but I take the present opportunity as a fitting time to say that as an old telegraph man, not only in this country but in many parts of the world, I consider that the Post Office have carried out the work they undertook in an admirable manner, and as regards the performance of internal telegraph work have carried it to greater perfection than it has reached in any Continental countries or even America.

Mr. Graves, however, will allow me to correct him in his statement that there were no sound instruments in this country in 1869. Nearly the whole of the Magnetic Company's system was worked by sound instruments, comprised in my own patents of 1852 and 1855, and the service was carried out with great speed and accuracy. I wish that Mr. Graves had extended his paper so

as to give us details as to foreign messages, and I should be much interested to have his opinion of the probable result of the adoption of the word rate to the Continent, which was introduced on the 1st of April last.

I beg, in conclusion, to propose a hearty vote of thanks to Mr. Graves, for the very excellent paper and details he has given us.

Lieut.-Col. WEBBER, R.E., was glad that such a valuable paper as Mr. Graves' had been brought before the Society. He had observed that during the Paris exhibition of 1878 (at which he was a juror) statistical tables had been shown by the various countries represented which gave much interesting information to visitors in regard to telegraphs. Mr. Graves' paper was the first compilation he had seen of general statistics of the progress of telegraphs in England. He could appreciate the value of that which such statistics represented rather more, perhaps, than one looking at them from an ordinary telegraphist point of view. His connection with the Post Office had ceased for something like twelve months, and such an interval having elapsed he was placed more in the position of an unprejudiced observer of the great work carried out during the nine previous years, which he had always looked upon as some of the most interesting years of his life.

Those nine years, too, were of especial interest to the members of the Society, because it was the activity in the telegraph world of England, caused by the extensive operations of the Post Office, that had tended much to the development of the Society. The principal and many of the subordinate officers of the government telegraphs were members of the Society, which was no small fact for congratulation. The very fact that the personnel of the old Telegraph Companies had been blended with a civil department of the State was no mean achievement, and it was pleasant to reflect that the hearty energy, hard work, and good will of every one concerned had brought about such grand results. These remarks, if inserted in the Society's journal might be considered as mere compliments, but the fact was, that he could not allow such an opportunity to pass without bearing witness from another point of view to the great results brought out by the paper. The con-

cluding words of the paper were "progress," "onward," and he hoped, in his present position as an ordinary member of the British public, that the onward march of progress of that valuable public department would result, at any rate, in the next "decade," in sixpenny telegrams of one sort or another. Such a rate, whether for a large or small number of words, would place the telegraph at the disposal of a much larger section of the public than that which now enjoys its advantages.

Mr. C. E. SPAGNOLETTI seconded the vote of thanks, and remarked that to compile a paper of this description was a very great task, and tax on a busy man's time, but that the result was only another evidence of the competency of Mr. Graves to deal with such statistical subjects; experience of which had no doubt been felt by most railway companies who had arbitrated for compensation with the Post Office, and which had produced advantageous results to that Department.

Mr. ROBERT GRAY replied to Sir Charles Bright, as to the result of the adoption of the single word tariff, that in the case of the duplicate cable recently laid between Marseilles and Algiers, the Minister of Telegraphs in France reduced the rate from 2d. to 1d. per word, with the immediate result that, where 800 messages a-day had been previously passed over the original line, about 1,200 was the present daily number. Taking 13 words each as the average message, a very good return was shown on the capital expended.

The PRESIDENT put the vote of thanks, which was unanimously carried.

Mr. E. GRAVES: Before thanking you for the vote you have been good enough to accord, may I be allowed to put myself right on one or two matters of fact? Sir Charles Bright called my attention to what he considered an error. Perhaps my friend will forgive me pointing out that there is really no error. My object has been to contrast the facts of 1869 with those of 1879. The Bright's Bell instrument——

Sir CHARLES BRIGHT: Acoustic telegraph.

Mr. GRAVES: The familiar name was the Bright's Bell; and that instrument was used in 1869, and is continued to an equal

extent in use in 1879. Where I have erred is in speaking of the sounder (which is the term used in ordinary parlance), instead of the "Morse" sounder. The novel introduction is that of the sounder instrument, formerly used only in America, which is in fact a Morse instrument without the recording apparatus. That was mentioned as a new thing, and I did not speak of the old forms of instrument that continue to be used.

With regard to submarine and foreign telegraphs, I do not like to make promises. I have found that very much work is involved in the compilation of statistics such as have been now supplied, and I must hesitate before making a definite promise to extend the compilation so as to cover the statistics of foreign telegraphs completely; although I trust that sometime during next winter I may be able to gather together facts, not only with reference to the extensions and traffic of the cable companies, but also of telegraphs throughout the Continent, America, and the Colonies.

As to anticipations of the future I will say nothing at all. No doubt the future has a great deal to bring forth, and no doubt also Col. Webber's anticipations will be justified when the time comes; but as I cannot tell him when the time will come, and as I cannot tell him the precise *modus operandi* that may be put in force to bring it about, I will say nothing at all about it. I thank you for the kindness with which you have received my paper.

The PRESIDENT: I do not think that there is anybody more competent to deal with this subject that Mr. Graves, and we are all pleased to hear the probability of a supplementary paper being forthcoming before the Society after the recess. I am much interested in the preparation of papers for the autumnal session; and, if we are favoured with such a paper by Mr. Graves as he has sketched forth, I am sure that the facts of external telegraphy will prove quite as startling as those of internal telegraphy we have just heard. In fact, as I have once before mentioned in this hall, the mileage of cables extending over the globe is something marvellous. Mr. Graves has brought before us facts that show that 2,000 offices in 1869 have increased to 5,400 offices in 1879; that 6,000,000 messages have increased to 27,000,000 messages; and that the income from telegraphs in 1869 of £700,000 has increased

to £1,400,000 in 1879. But perhaps more surprising than all is the prominent fact brought out, that the apparatus used by the railway companies is four times greater in number than that used by the Post Office; for while of speaking, block, and repeating apparatus, the railway companies have 46,847, the Post Office employs only 12,000 instruments.

Every railway has its own distinct organization; and there are many officers in connection with railways who might bring papers before us, describing in detail, as Mr. Graves has done to-night, the organization and statistics in connection with railway telegraphs.

Another aspect to the question is, that while we look eagerly into the future for further statistics, and to make our records accurate, we ought to have something more of the past. There are several members present who have been intimately connected with the growth of telegraphy from its introduction in England to the present time, and I shall certainly before the autumnal session call upon Sir Charles Bright to favour us with some records, which I know he must possess or can obtain, which will make the historical description of the growth of telegraphy in this country more complete, and then I think, what with the history of the past, the history of the present, and favours to come in the history of external telegraphy, we may say that, at any rate, during the present year the Society of Telegraph Engineers have placed on the records of their proceedings the most valuable and useful facts in the progress of telegraphy.

I am now glad to announce that the several-times-promised paper by Dr. Siemens will in every reasonable certainty be brought forward on Thursday evening, June 3rd, which is a special extra night set apart for the purpose, and until which date this meeting will now be adjourned.

A Special General Meeting of the Society was held on Thursday Evening, June 3rd, 1880, at the Institution of Civil Engineers, 25, Great George Street, Westminster—Mr. W. H. PREECE, President, in the chair.

After the usual formal proceedings,

The PRESIDENT said that it was with very great pleasure that he called upon Dr. C. W Siemens to give his paper, for which the special meeting had been called.

Dr. C. W. SIEMENS, D.C.L., LL.D., F.R.S.: I have to commence my address with an explanation and also with an apology; an explanation to justify myself for not bringing my paper before this Society when promised. The reason is, that I have lately been taken ill, and unable to go through the task of reading the paper or describing experiments and this fact leads me to the apology, which is, that I am sorry to say I must ask the Secretary to read the paper to certain points, at which I will reserve for myself the explanations of the diagrams and apparatus before you.

## THE DYNAMO-ELECTRIC CURRENT IN ITS APPLICATION TO METALLURGY, TO HORTICULTURE, AND TO LOCOMOTION.

By C. WILLIAM SIEMENS, D.C.L., LL.D., F.R.S.

In the inaugural address which I had the honour to deliver to the Society of Telegraph Engineers on the 23rd January, 1878, I called special attention to the applicability of the dynamo-electric current for purposes beyond the range of what electricity had theretofore been employed in effecting. Among these purposes I specially referred to the transmission of power, and to the accomplishment of large chemical results, such as the decomposition of metallic salts, &c.

My object to-day is to corroborate my statement by describing some further experimental results obtained by means of the dynamo-electric current, which I hope will prove to be of interest.

So long as the electric current was produced by the decomposition of zinc in a galvanic battery, it was too expensive a form of energy to do our behests in the way of massive effects, it was

resorted to principally when energy, in the form of heat or mechanical power, failed to accomplish our ends; and the rapid rate at which electrical energy passes through a metallic conductor rendered it the only available means of producing instantaneous effects at great distances. The dynamo-electric current, on the other hand, provides us with the means of transforming mechanical energy into electric energy without much loss, and it enables us further to accumulate electrical impulses in such a manner as to produce mechanical effects measurable not by grains or ounces, but by pounds and tons of weight lifted through a considerable height.

It is not my present intention to enter into particulars regarding the dynamo-electric machine, but to describe three somewhat novel applications of it, viz., as a means of effecting the fusion of refractory materials in considerable quantities in an electric furnace; in its effects upon horticulture, as a promoter of the chemical changes by which the plant takes its chief ingredients of food from the atmosphere; and as a means of mechanical propulsion, in which the dynamo-electric current enters the list as a rival of steam, to work either stationary machinery, hoists, or lifts, or to propel trains along rail or tramways.

## ON THE APPLICATION OF THE DYNAMO-ELECTRIC CURRENT TO THE FUSION OF REFRACTORY MATERIALS IN CONSIDERABLE QUANTITIES.

Amongst the means at our disposal for effecting the fusion of highly refractory metals, and other substances, none has been more fully recognised than the oxy-hydrogen blast. The ingenious modification of the same by M. H. Ste.-Claire Deville, known as the Deville furnace, has been developed and applied for the fusion of platinum in considerable quantities by Mr. George Matthey, F.R.S.

The Regenerative Gas Furnace furnishes, however, another means of attaining extremely high degrees of heat, and this furnace is now largely used in the arts—among other purposes, for the production of mild steel. By the application of the open hearth process, 10 to 15 tons of malleable iron, containing only traces of carbon or other substances alloyed with it, may be seen in a perfectly fluid condition upon the open hearth of the furnace,

at a temperature probably not inferior to the melting point of platinum. It may be here remarked that the only building material capable of resisting such heats is a brick composed of 98·5 per cent. of silica, and only 1·5 per cent. of alumina, iron, and lime, to bind the silica together.

In the Deville furnace, an extreme degree of heat is attained by the union of pure oxygen with a rich gaseous fuel under the influence of a blast, whereas in the Siemens furnace it is due to slow combustion of a poor gas, potentiated, so to speak, by a process of accumulation through heat stores or regenerators.

The temperature attainable in both furnaces is limited by the point of complete dissociation of carbonic acid and aqueous vapour, which, according to Ste.-Claire Deville and Bunsen, may be estimated at from 2,500° to 2,800° C. But long before this extreme point has been reached, combustion becomes so sluggish that the losses of heat by radiation balance the production by combustion, and thus prevent further increase of temperature.

It is to the electric arc, therefore, that we must look for the attainment of a temperature exceeding the point of dissociation of products of combustion, and indeed evidence is not wanting to prove the early application of the electric arc to produce effects due to extreme elevation of temperature. As early as the year 1807, Sir Humphrey Davy succeeded in decomposing potash by means of an electric current from a Wollaston battery of 400 elements, and in 1810 the same philosopher surprised the members of the Royal Institution by the brilliancy of the electric arc produced between carbon points through the same agency.

Magneto-electric and dynamo-electric currents enable us to produce the electric arc more readily and economically than was the case at the time of Sir Humphrey Davy, and this comparatively new method has been taken advantage of by Messrs. Huggins, Lockyer, and other physicists, to advance astronomical and chemical research with the aid of spectrum analysis. Professor Dewar quite recently, in experimenting with the dynamo-electric current, has shown that in his lime tube or crucible several of the metals assume the gaseous condition, as demonstrated by the reversal of the lines in his spectrum, thus proving that the temperature attained was not much inferior to that of the sun.

My present object is to show that the electric arc is not only capable of producing a very high temperature within a focus or extremely contracted space, but also such larger effects, with comparatively moderate expenditure of energy, as will render it useful in the arts for fusing platinum, iridium, steel, or iron, or for effecting such reactions or decompositions as require for their accomplishment an intense degree of heat, coupled with freedom from such disturbing influences as are inseparable from a furnace worked by the combustion of carbonaceous material.

The apparatus which I employ to effect the electro-fusion of such material as iron or platinum is represented in the accompanying drawing, fig. 1. (This diagram was explained in full detail to the meeting.) It consists of an ordinary crucible of plumbago or other highly refractory material, placed in a metallic jacket or outer casing, the intervening space being filled up with pounded charcoal or other bad conductor of heat. A hole is pierced through the bottom of the crucible for the admission of a rod of iron, platinum, or dense carbon, such as is used in electric illumination. The cover of the crucible is also pierced for the reception of the negative electrode, by preference a cylinder of compressed carbon of comparatively large dimensions. At one end of a beam supported at its centre is suspended the negative electrode by means of a strip of copper, or other good conductor of electricity, the other end of the beam being attached to a hollow cylinder of soft iron free to move vertically within a solenoid coil of wire, presenting a total resistance of about 50 units or ohms. By means of a sliding weight the preponderance of the beam in the direction of the solenoid can be varied so as to balance the magnetic force with which the hollow iron cylinder is drawn into the coil. One end of the solenoid coil is connected with the positive, and the other with the negative pole of the electric arc, and, being a coil of high resistance, its attractive force on the iron cylinder is proportional to the electro-motive force between the two electrodes, or, in other words, to the electrical resistance of the arc itself.

The resistance of the arc was determined and fixed at will within the limits of the source of power, by sliding the weight upon the beam. If the resistance of the arc should increase from

any cause, the current passing through the solenoid would gain in strength, and the magnetic force overcoming the counteracting weight, would cause the negative electrode to descend deeper into the crucible; whereas, if the resistance of the arc should fall below the desired limit, the weight would drive back the iron cylinder within the coils, and the length of the arc would increase, until the balance between the forces engaged had been re-established.

Experiments with long solenoid coils have shown that the attractive force exerted upon the iron cylinder is subject only to slight variation within a range of several inches, that is, within the limits when the iron cylinder has just entered the coil, and when it has advanced a little beyond the point of half immersion, which circumstance allows of a range of several inches of nearly uniform action on the electric arc. The accompanying diagram, fig. 2, represents the attractive force of a solenoid coil of this description upon its iron core, the abscissæ representing the depth of immersion of the uppermost end of the iron in the coil in centimetres, and the ordinates the attractive force in grams.

This automatic adjustment of the arc is of great importance to the attainment of advantageous results in the process of electric fusion; without it the resistance of the arc would rapidly diminish with increase of temperature of the heated atmosphere within the crucible, and heat would be developed in the dynamo-electric machine to the prejudice of the electric furnace. The sudden sinking or change in electrical resistance of the material undergoing fusion would, on the other hand, cause sudden increase in the resistance of the arc, with a likelihood of its extinction, if such self adjusting action did not take place.

Another important element of success in electric fusion consists in constituting the material to be fused the positive pole of the electric arc. It is well known that it is at the positive pole, that the heat is principally developed, and fusion of the material constituting the positive pole takes place even before the crucible itself is heated up to the same degree. This principle of action is of course applicable only to the melting of metals and other electrical conductors, such as metallic oxides, which constitute the materials generally operated upon in metallurgical processes. In *operating* upon non-conductive earth or upon gases, it becomes

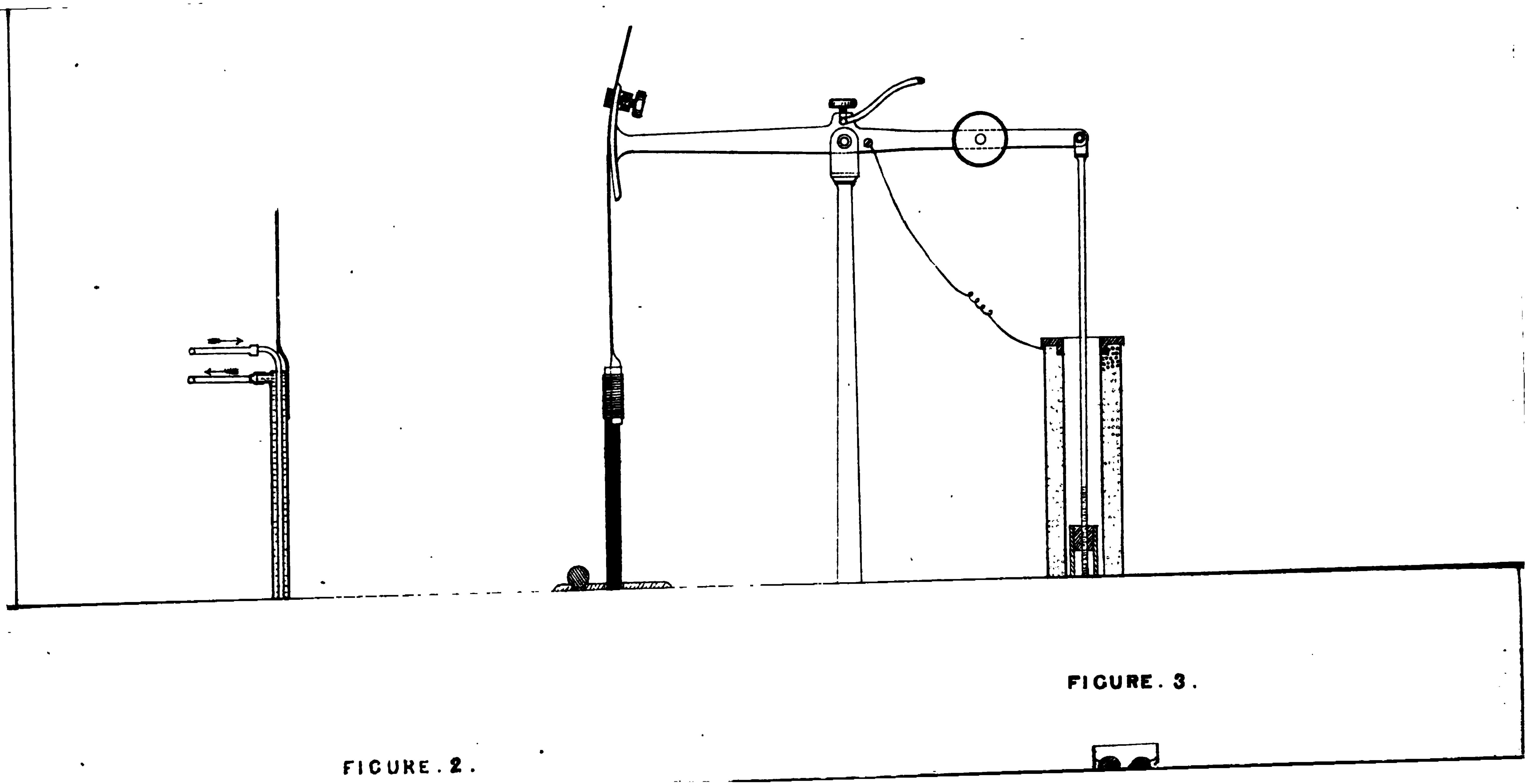


FIGURE. 2.

FIGURE. 3.

ecessary to provide a non-destructible positive pole, such as platium or iridium, which may, however, undergo fusion, and form a ttle pool at the bottom of the crucible.

In this electrical furnace some time, of course, is occupied to ring the temperature of the crucible itself up to a considerable egree, but it is surprising how rapidly an accumulation of heat kes place. In working with the modified medium-sized dynamo achine, capable of producing 36 webers of current with an xpenditure of 4 horse-power, and which, if used for illuminating urposes, produces a light equal to 6,000 candles, I find that a rucible of about 20 centimetres in depth, immersed in a nononductive material, is raised up to a white heat in less than quarter of an hour, and the fusion of 1 kilogram of steel is ffected within say, another quarter of an hour, successive fusions eing made in somewhat diminishing intervals of time. It is quite easible to carry on this process upon a still larger scale by ncreasing the power of the dynamo-electric machine and the size f the crucibles. (These remarks were illustrated by an actual rrangement on the plan described, and 1 lb. weight of broken les, which were placed in a crucible through which a dynamourrent of about 70 webers was passed, was brought to a liquid tate in thirteen minutes, and poured out of the crucible in that ondition.)

By the use of a pole of dense carbon, the otherwise purely hemical reaction intended to be carried into effect may be nterfered with through the detachment of particles of carbon from he same; and although the consumption of the negative pole in a eutral atmosphere is exceedingly slow, it may become necessary to ubstitute for the same a negative pole so constituted as not to yield ny substance to the arc. I have used for this purpose (as also in e construction of electric lamps) a water pole, or tube of copper, rough which a cooling current of water is made to circulate. t consists simply of a stout copper cylinder closed at the lower d, having an inner tube penetrating to near the bottom for the ssage of a current of water into the cylinder, which water enters d is discharged by means of flexible india-rubber tubing. This bing being of non-conductive material, and of small sectional ea, the escape of current from the pole to the reservoir is so

slight that it may be entirely neglected. On the other hand, some loss of heat is incurred through conduction in the use of the water pole, but this loss diminishes with the increasing heat of the furnace, inasmuch as the arc becomes longer, and the pole is retired more and more into the crucible cover.

The dynamo-electric machine consuming 4·25 horse-power, or 3·17 ergtens, per second, will send a current of 40·5 webers through 1 unit electrical resistance; replacing the resistance by an arc maintained by the balance-weight constantly at 37·8 volts electro-motive force, the same current will flow.

Neglecting the connecting wires, there will be developed in the arc an energy of

$$1{,}531{\cdot}2 \times 10^7 \text{ ergs. per second} = 9{,}187{\cdot}2 \times 10^8 \text{ ergs. per minute,}$$

$$\text{or } 1{,}378{\cdot}1 \times 10^{10} \text{ ergs. per 15 minutes} = 32{\cdot}8 \times 10^4 \text{ gram water degree units of heat.}$$

Assuming steel to have the same specific heat as iron, viz., at $t^0$ =

$$0{\cdot}1040 \times 0{\cdot}000144\, t^0,$$

and that the melting point of steel is 1800° C., then 420·5 units of heat will be expended in raising the steel to this temperature; and assuming the latent heat of fusion of steel at 29·5 units (silver is 21, and zinc 28), there are roughly 450 units required to melt a gram of steel, and 225,000 to melt half a kilogram, that is about $\frac{2}{3}$ of the heat generated in the crucible, and $\frac{1}{3}$ of the horse-power actually expended. A good expansive condensing steam engine converts the heat energy residing in coal into mechanical energy, with a loss of over eighty per cent., or, in other words, $\frac{1}{6}$ only of the 7,000 units residing in a gram of ordinary coal is represented as work in the engine. It hence follows that the useful effect attainable in the electric furnace is $\frac{1}{3} \times \frac{1}{6} = \frac{1}{18}$ of the heat energy residing in the fuel consumed under the boiler of the engine.

To melt a gram of steel in the electric furnace takes, therefore $450 \times 18 = 8{,}100$ units, which is within a fraction the heat actually contained in a gram of pure carbon. It results from this calculation that, through the use of the dynamo-electric machine worked by a steam engine, when considered theoretically, one

pound of coal is capable of melting nearly one pound of mild steel. To melt a ton of steel in crucibles in the ordinary air furnace used at Sheffield, from 2½ to 3 tons of best Durham coke are consumed, the same effect is produced with one ton of coal when the crucibles are heated in the Regenerative Gas Furnace, whilst to produce mild steel in large masses on the open hearth of this furnace, 12 cwt. of coal suffice to produce one ton of steel. The electric furnace may be therefore considered as being more economical than the ordinary air furnace, and would, barring some incidental losses not included in the calculation, be as regards economy of fuel nearly equal to the Regenerative Gas Furnace.

It has, however, the following advantages in its favour :—1st. That the degree of temperature attainable is theoretically unlimited. 2nd. That fusion is effected in a perfectly neutral atmosphere. 3rd. That the operation can be carried on in a laboratory without much preparation, and under the eye of the operator. 4th. That the limit of heat practically attainable with the use of ordinary refractory materials is very high, because in the electric furnace the fusing material is at a higher temperature than the crucible, whereas in ordinary fusion the temperature of the crucible exceeds that of the material fused within it.

Without wishing to pretend that the electric furnace here represented is in a condition to supersede other furnaces for ordinary purposes, the advantages above indicated will make it a useful agent, I believe, for carrying on chemical reactions of various kinds at temperatures and under conditions which it has hitherto been impossible to secure. (A second charge of steel was now put into the crucible, and was poured out in a molten state at the end of eight minutes.)

THE EFFECT OF DYNAMO-ELECTRIC ENERGY UPON HORTICULTURE, AS A PROMOTER OF THE CHEMICAL CHANGES BY WHICH THE PLANT TAKES ITS CHIEF INGREDIENTS OF FOOD FROM THE ATMOSPHERE.

A consideration of the extremely elevated temperature of, and of the effects produced in experimenting with, powerful electric arcs, such as causing blistering of the skin and a feeling

sunstroke in the incautious observer, has led me to reflect whether the action of the arc was not analogous to that of the sun in its effect also upon vegetable life. The solar ray, in falling upon the leaf of a plant, not only produces the colouring matter called chlorophyll, but effects within the vegetable cell decomposition of the carbonic acid and aqueous vapour absorbed from the atmosphere for the formation of starch and woody fibre.

I mentioned my views on this subject to several botanists, from whom I received some encouragement to put the question to practical test, which I accordingly did, commencing in the early portion of the present year, at my country residence, of Sherwood, near Tunbridge Wells.

The apparatus I use consists:—1st, of a vertical Siemens dynamo-machine, weighing 50 kilograms, with a resistance of 0·717 unit on the electro-magnets. This machine makes 1,000 revolutions a minute; it takes two horse-power to drive it, and develops a current of from 25 to 27 webers of an intensity of 70 volts. 2. A regulator or lamp, constructed for continuous currents, with two carbon electrodes of 12 mm. and 10 mm. diameter respectively. The light produced is equal to 1,400 candles. 3. A three horse-power Otto gas engine as motor.

My object was to ascertain by experiment whether electric light affected the growth of plants. For this purpose I placed in the open air a regulator contained in a lamp having a metallic reflector, about 2 metres above the roof of a sunk melon house. Several pots were provided, and planted with quick-growing seeds and plants, such as mustard, carrots, melons, etc. The plants were arranged to be brought at suitable intervals, without moving them, under the influence of daylight and electric light, both falling upon them at approximately the same angle. The pots were divided into four groups or series. One group was kept entirely in the dark, another was exposed to the influence of electric light only, the third to the influence of daylight only, and the fourth was exposed successively to both day and electric light.

In this first trial the electric light was supplied during six hours, from 5 to 11 each evening, the plants being left in darkness the rest of the night, but in experiments hereafter to be referred to, the electric light was kept on during the whole night.

In every instance the differences of effect were unmistakable. The plants kept in the dark were pale yellow, thin in the stalk, and soon died. Those exposed to electric light only, showed a light green leaf, and had sufficient vigour to survive. Those exposed to daylight only were of a darker colour and greater vigour. Those exposed to both sources of light evinced a decided superiority in vigour over the rest, and the colour of the leaf was a dark rich green.

It must be remembered that in this trial of electric against solar light, the period of exposure was in favour of the latter in the proportion of nearly 2 to 1, but after making every allowance, the average daylight in these latitudes in the early portion of the year appears to have about twice the effect of electric light. It was evident, however, that the electric arc was not so placed as to give out its light to the greatest advantage. The nights were cold, and the plants under experiment were for the most part of a character to require a hot moist atmosphere; the glass thus became covered very thickly with moisture, obstructing thereby the action of the light, besides which the electric light had to traverse the glass of its own lamp. Notwithstanding these drawbacks, the electric light clearly formed chlorophyll and its derivatives in the plants. But it was, besides, interesting to observe the mechanical action that took place, for the mustard seed stem, when placed obliquely, turned completely towards the light in the course of two or three hours, and the stems of cucumber and melon plants also did so, though more slowly. The cucumber and melon plants which have been exposed to both day and electric light have made great progress, and my gardener tells me that he could not have brought on the latter without the aid of the electric light during the early winter. These preliminary trials go to prove that electric light can be called to the aid of solar light by using it outside of green houses, but the loss of effect in such cases is considerable.

I next directed my observations to the effect of electric light upon plants, when both were placed in the same enclosure. A portion of the melon-house already referred to was completely darkened with a covering of thick matting, and was whitewashed

inside. The electric lamp was placed over the entrance door, and shelves were arranged in a horse-shoe form, with pots containing the plants to be experimented upon, the plants being placed at distances from the source of light varying from 0·5 metre to 2·3 metres. The first time the naked electric light was tried in this manner, some of the plants, and especially some melon and cucumber plants, from 20 cm. to 40 cm. in height, less than a metre distance from the lamp, suffered, the leaves nearest the lamp turning up at the edges, and presenting a scorched appearance. In the later experiments the stands were so arranged that the distance of the plants from the light was from 1·5 to 2·3 metres. The plants were divided into three groups; one group was exposed only to daylight, a second group received only electric light during eleven hours of the night, being in darkness during the day, and the third group had the benefit of 11 hours day and 11 hours electric light. These experiments were continued during four consecutive days and nights, and the results are very striking and decisive as regards the effect upon such quick growing plants as mustard, carrots, &c. The trial was unsatisfactory in this one respect, that during the third night the gas-engine working the dynamo machine came to a stand-still, owing to a stoppage in one of the gas channels, and the electric light was only applied half the night. Notwithstanding this drawback the plants were evidently benefited by the electric light. The plants that had only been exposed to daylight (with a fair proportion of sunlight) presented the usual healthy green appearance; those exposed to electric light alone were of a somewhat lighter hue than those exposed to daylight, except in one instance when the reverse was the case; while the plants that had the benefit of both day and electric light far surpassed the others in darkness of green and general vigour. A fear had been expressed that the melon and cucumber places which had been scorched by the electric light on the first evening would droop or die under continued exposure to that agency, but they were replaced at a distance from the light exceeding 2 metres, and they have all shown signs of recovery. A pot of tulip buds was placed in this electric stove, and the flowers opened completely after an exposure of two hours.

Another object I had in view in this experiment was to observe whether the plants were injured by carbonic acid, and the nitrogenous compounds observed by Professor Dewar to be produced within the electric arc. All continuous access of air into the stove was stopped, and in order to prevent excessive accumulation of heat, the stove pipes were thickly covered over with matting and wet leaves. But although the access of stove heat was thus stopped, the temperature of the house continued through the night at 72° F., proving that the electric light furnished not only light, but sufficient heat also. No injurious effect was observed on the plants from the want of ventilation, and it is probable that the supply of carbonic acid given off by the complete combustion of the carbon electrodes at high temperature, and under the influence of an excess of oxygen, sustained their vital functions. If nitrogenous compounds were produced in large quantities, it is likely the plants would have been injured; but they could not be perceived by their smell in the stove, when all ventilators were closed, and no injurious effects on the plants have been observed.

These experiments are instructive in proving that electric light alone promotes vegetation, and the important fact that diurnal repose is unnecessary to plant life, although the experiments have perhaps not lasted long enough to furnish that proof absolutely. We may argue, however, from analogy, that such repose is not necessary, as crops grow and ripen very quickly in northern latitudes, where the summer is only two months in length, during which period the sun is almost altogether above the horizon.

I next removed the electric light into a palm house constructed of framed glass (8·6 m. × 14·4 m. × 4·42 m.) In its centre, a banana palm and a few other small palms are planted, whilst a considerable variety of flowering plants are placed around the interior. The electric light was placed at the south corner of the house, as high as practicable, that its rays might fall upon the plants in the same direction and at the same angle as those of the sun during the middle of the day. A metallic reflector was placed behind the lamp, so as to utilise all the rays as far as possible. Some young vines are planted along the eastern side of the house. Three pots of nectarines just on the

bud were placed on the floor at different distances from the light, and also some roses, geraniums, orchids, etc. The temperature of the house was maintained at 65° F., and the electric lamp was kept alight from 5 p.m. to 6 a.m. for one week, from February 18th to February 24th, excepting Sunday night. The period of the trial was hardly sufficient to produce very striking effects, but the plants remained healthy. The vine nearest the light made most progress, and the same statement could be made regarding the nectarines and roses. Other plants, such as geraniums, continued to exhibit a vigorous appearance, and the electric light appeared to impart the vitality necessary to prevent the plants being injured through excessive temperature. This experiment is important in showing that the electric light, when put into conservatories, improves the appearance and growth of the plants—the leaves become darker, the plants more vigorous, and the colouring of the flowers brighter; but a further period of time is necessary to establish this observation absolutely. The effects produced by electric light in conservatories are very striking, owing to the clearer definition of form and colour due thereto.

I decided in the next place to try the effect of the electric light upon plants in the open air and under glass at the same time. The regulator was returned to its first position, two metres above the ground, with a sunken melon house on the one side, and a sunken house containing roses, lilies, strawberries, and a variety of other plants on the other. Upon the ground between these were placed boxes sown with early vegetables, and protecting walls were erected across the openings of the passage between the two houses, in order to protect the plants from cold winds. The effects could thus be simultaneously observed upon the melons and cucumbers in the one house, upon the roses, strawberries, etc., at a lower temperature, in the other, and upon the early vegetables unprovided with covering.

That growth was promoted under all these varying circumstances I proved clearly, by shading a portion of the plants both under glass and in the open air from the electric light, without removing them from their position of equal temperature, and ex-

posing them to solar light during day time. Upon flowering plants the effects are very striking, and the electric light is apparently more efficacious to bring them forward than daylight in winter. Although the quantity of heat given off by the electric light is not so great in amount as that from burning gas, yet the heat rays from the arc counteract that loss of heat from the leaves by radiation into space, which causes hoar frost on a clear night. An experiment made during a night of hoar frost clearly proved that although the temperature on the ground did not differ materially within the range of the electric light and beyond it, the radiant effect of the light was such as to prevent frost entirely within its range. For this reason I anticipate the useful application of electric light in front of fruit walls, in orchards, and in kitchen gardens, to save the fruit bud at the time of setting.

Considering the evident power of the electric light to form chlorophyll, there seemed reason to suppose that its action would also in the case of ripening fruit resemble that of the sun, and that saccharine matter, and more especially the aromatic constituents, would be produced. To test this opinion practically, several plants of early strawberries in pots were placed, as in the last experiment, in two groups, the one being subjected to daylight only, and the other to solar light during the day and to electric light at night. Both groups were placed under glass, at temperatures varying from 65° to 70° F., those that received daylight only being shielded from the effect of the electric light.

At the commencement of the experiment the strawberry plants were partly setting fruit, and partly in bloom. After a week the fruit on the plants exposed to electric light had swelled very much more than that on the others, some of the berries showing signs of ripening. The experiment was interrupted for two nights at this stage, and when the electric light was resumed, very rapid progress was observable; and after four days continuous exposure to both day and electric light, the majority of the berries had become ripe, and showed a rich red colour, while the fruit on the plants that had been exposed to daylight only, scarcely showed even a sign of redness.

Melons that have been brought forward with the aid of the

electric light also clearly prove its advantageous effects in promoting the setting of fruit, the process of ripening, and the production of aroma.

These experiments go to prove that electric light is efficacious in promoting the formation of fruit rich in bloom and aroma, and if these results should be confirmed, the horticulturist will be able to make himself practically independent of solar light for producing a high quality of fruit at all seasons of the year.

Although I have shown that a maximum beneficial result on vegetation is produced at a distance of 2 metres with a lamp of 1,400 candle power, the influence is very marked upon plants at greater distances. This action at a distance was proved by the condition of three melon plants situated towards the back of the house, which throve remarkably well for about a fortnight that the electric light was in front of the house, at a distance of from 5 to 6 metres from the plants. After the electric light was removed in front of the other end of the same house, and the plants in question were deprived of its influence, they continued their growth, but have shown a very decided falling-off in vigour.

An important consideration is the cost of electro-horticulture. This depends upon the cost of the fuel or other source of energy, and upon the scale of application. To work one electric lamp only with a small steam or gas engine is expensive, both as regards fuel and cost of attendance. If steam has to be resorted to, it is important to employ an engine of sufficient size to produce economical results per horse-power of energy expended, and the electric arc should be of sufficient brilliancy to give a good effect for the power employed. Experience in electric illumination has established a form and size of machine both convenient and suitable for the attainment of economical results, viz., the medium dynamo-electric machine, which, if applied to a suitable regulator, produces fully 6,000 candle-power of diffused light with an expenditure of 4 horse-power. The experiments already referred to show that the most efficient height at which to place the naked electric arc of 1,400 candle-power is about 2 metres. By using a metallic reflector, the major portion of the upward rays may be thrown down upon the surface to be illuminated, and that height may be taken at

3 metres. If an electric arc of 6,000 candles was employed, the height would be $\frac{\sqrt{6000}}{\sqrt{1400}} \times 3 = 6{\cdot}2$ metres, at which such an electric light should be fixed. In operating upon an extended surface, several lamps should be so placed as to make the effect over it tolerably uniform. This would be so if the radiating centres were placed at distances apart equal to double their height above the ground; for a square foot of surface midway between them would receive from each centre one-half the number of rays falling upon such a surface immediately below a centre. A plant at the intermediate point would, however, have the advantage of presenting a larger leaf surface to both sources of light; and to compensate for this advantage, the light centres may be placed yet further apart, say at distances equal to 3 times their elevation, or 18 metres. Nine lights so placed would suffice for an area 54 metres square, or about $\frac{3}{4}$ acre. If a high fruit wall were to enclose this space, this will also get the full benefit of electric radiation, and would serve at the same time to protect the plants from winds. By subdividing the area under forced cultivation by vertical partitions of glass, as has been done with excellent results by Sir William Armstrong, protection is insured against injury from this latter cause.

There would be required to maintain this radiant action a $9 \times 4 = 36$ horse-power engine, involving the consumption of $36 \times 2\frac{1}{2} = 90$ pounds of fuel per hour, which, for a night of 12 hours, with 40 pounds for getting up steam, amounts to half-a-ton, costing, at 16s. a ton, 8s. a-night. This does not include, however, the cost of carbons, or of an attendant, which would probably amount to as much more, making a total of 16s. If, however, an engine could be utilised doing other descriptions of work during the day, the cost of steam power and attendance for the night work only would be considerably reduced.

I have assumed in the calculation just given the use of fuel to produce mechanical energy, but the question will assume a totally different aspect if natural sources of power, such as waterfalls, can be rendered available within a short distance. The cost of power will in such case be almost entirely saved, and that of

attendance greatly diminished, and it seems probable that under such circumstances electro-horticulture may be carried out with considerable advantage.

In reply to questions that have been frequently asked regarding the cost of maintaining an experimental electric light of 1,400 candle-power, such as I have used in these experiments, I may state that the 3 horse-power Otto gas engine employed in driving the dynamo machine consumes nearly 900 cubic feet of gas during the night of twelve hours, or 75 cubic feet an hour, which, at 3s. 6d. per 1,000 cubic feet, represents with the carbons a cost of 5d. an hour. This, however, does not include superintendence or incidental expenses, the amount of which must depend upon the circumstances of each case.

The experiments furnish proof that no particular skill is required in the management of the electrical apparatus, as the gas engine, dynamo machine, and regulator have been under the sole management of my head gardener, Mr. D. Buchanan, and of his son, an assistant gardener. The regulator only requires the replacement of carbons every four or five hours, which period may easily be extended to twelve hours, by a slight modification of the lamp.

I am led to the following conclusions as the result of my experiments:—

1. That electric light is both efficacious in producing chlorophyll in the leaves of plants and in promoting growth.

2. That a light-centre equal to 1,400 candles, placed at a distance of two metres from growing plants, appears to be equal in effect to average daylight in February, but more economical effects can be attained by more powerful light-centres.

3. That carbonic acid, and the nitrogenous compounds generated in diminutive quantities in the electric arc produce no sensible deleterious effects upon plants enclosed in the same space.

4. That plants do not appear to require a period of rest during the 24 hours of the day, but make increased and vigorous progress if subjected during daytime to sunlight and during the night to electric light.

5. That the radiation of heat from powerful electric arcs can be made available to counteract the effect of night frost.

6. That while under the influence of electric light, plants can sustain increased stove heat without collapsing, a circumstance favourable to forcing by electric light.

7. That the light is efficacious in hastening the development of flowers and of fruit; the flowers produced by its aid are remarkable for intense colouring, and the fruit both for bloom and aroma, without apparent augmentation of the saccharine constituents.

8. That the expense of electro-horticulture depends mainly upon the cost of mechanical energy, and is very moderate when natural sources of such energy, as waterfalls, can be made available.

Some observations made by Dr. Schübeler, of Christiania, to which my attention has been drawn, fully confirm the conclusion indicated by my experiments with electric light. According to Dr. Schübeler, plants are able to grow continuously; and when under the influence of continuous light, they develop more brilliant flowers and larger and more aromatic fruit than when under the alternating influence of light and darkness.

The useful influence of the electric light in horticulture having been thus established, I have taken steps to test the principle upon a working scale. Natural sources, such as water power, not being available, I have had to resort to steam as the motive agent. With this object I have laid down a 6 horse-power horizontal engine, by Tangye Brothers, and a Cornish boiler, fitted with 2 Galloway tubes in the flue, close to the conservatories at Sherwood, and at a distance of somewhat less than a quarter-of-a-mile from the farm buildings. The power of this engine is sufficient to give motion to two dynamo machines, capable of producing 12,000 candle-power of light. The steam, after doing its work in the engine, will be made available as a heating agent for the hot-houses; but it having been found undesirable to pass such steam directly into the pipes leading to the houses, an intermediate tubular heater is used to effect the condensation of the steam, and to communicate its latent heat to the water circulating through the ranges of pipes in the usual manner. The fires now necessary to maintain the heat of the circulating pipes are suppressed, and that below the steam boiler substituted, which, admitting of an

arrangement more suitable for economical results, will, it is expected, require little or no additional expenditure of fuel beyond what has hitherto been necessary to work the stoves. The power required for the electric light can therefore, under the peculiar circumstances of the case, be obtained at a comparatively low cost during the time the electric light is needed. The stoves, however, will require firing during the daytime; and in order to utilise the engine power when not required for electric lighting, arrangements will be made so that the machines for chaff and turnip cutting at the farm, and also some wood-cutting machinery, shall be worked by electric transmission of power, the current being derived from the same dynamo-electric machine which works the electric light during the night. By means of these arrangements the subject of electro-horticulture will be put during the ensuing winter to the test, not only of what can be accomplished by its aid, but as to the cost at which those effects may be obtained.

A question of considerable interest connected with this subject is that of determining which portion of the rays constituting white light is efficacious in producing chlorophyll, starch, and woody fibre, and which in effecting the ripening of fruit. For this purpose arrangements are in preparation to distribute the spectrum of a powerful electric light in a darkened chamber over a series of similar plants exposed seriatim to the actinic, light-giving, and thermal portions of the spectrum. Some experiments have been made with solar light in this direction, but no very conclusive results could be obtained, because the short periods of time during which the solar spectrum can be maintained steadily in the same place is so short that the effects produced upon vegetation have not been of a sufficiently decided character; whereas, with the aid of electric light, the same spectrum may be kept on steadily for a series of days without intermission.

For this purpose, and for electro-horticulture generally, it is important to employ a lamp with its focus unchangeable in space, and without obstruction to the rays of light falling downward. The lamp which I have designed for this purpose, and which has already been referred to in the paper read before the Society by Mr. Alexander Siemens, is represented in the accompanying drawing.

The principal point of novelty involved in it consists in the mode of advancing the carbons, which, instead of being effected by clockwork (as has been the case hitherto in constructing regulators), is effected simply by the force of gravity, or by spring power urging the carbons forward towards the point of meeting, in which forward motion each carbon is checked by a metallic abutment, in the form of a point or edge of copper or other metal of high conductivity, the exact position of which can be regulated by a screw.

This metallic ridge touches the carbons laterally, at a distance of 10 to 15 millimetres from the luminous point, where the temperature of the carbon is sufficient to cause its gradual decomposition in contact with the atmosphere, without being high enough to fuse or injure the metal.

In the lamp before you, represented in fig. 3, the carbons are contained in horizontal holders, suspended from the lamp frame by means of four suspension rods: a solenoid coil is placed vertically above the point of light, the iron core of which is connected to the suspension rods on either side by means of rods, whereby horizontal motion is applied to the metallic carbon-holders, tending to separate them when the current flowing through the solenoid coil diminishes, and approaching the carbons when the current passing through the coil increases; the effect is, that an increased resistance in the electric arc causes a shortening, and a decrease, a lengthening of the arc itself. In order to steady the action of this regulator, the iron core carries a piston, working freely up and down in a vertical cylinder, having a throttled aperture for the ingress and egress of atmospheric air above the piston. The two metallic holders are put into conductive connection with the two wires leading up from the dynamo-electric machine, and the regulating solenoid coil is connected to the two holders respectively, so that the current active in this coil is always proportionate to the potential between the two electrodes, which by this arrangement is made practically constant. If this lamp is worked by alternating currents, the two carbons are made equal in diameter; but if worked by a continuous current, the carbon connected with the positive pole should be made larger than the one connected with the negative pole, in the ratio of about 3 : 2.

Instead of the solenoid regulator, the steel tape regulator may be employed, which I described in a paper read before the Royal Society on the 16th January, 1879, in reference to certain means of measuring and regulating electric currents. This thin strip or wire is in metallic connection at one end with one of the suspension rods connected with the positive pole, and at the other with one of the suspension rods connected with the negative pole, being led up and down over pullies in order to produce a total resistance of from 20 to 30 ohms. The tension on this wire produced by balance weights prevents the carbon points from coming into contact with each other. Whenever the current is turned on, the iron strip becomes heated and elongates, allowing the carbons to approach each other. From the moment they touch the arc is formed, causing less current to pass through the iron strip or by pass, which consequently contracts on cooling, and causes the carbon poles to separate, thus effecting the proper regulation of the arc.

In its application to horticulture, a metallic parabolic reflector of considerable diameter is placed over the luminous centre, in order to reflect downwards all the rays of light and heat which would otherwise pass upward, an arrangement which may be advantageously carried out in these lamps as used for illumination when placed at a considerable elevation above the ground.

The horizontal carbon-holders may be made of considerable length, and one rod of carbon may be made to follow up the preceding one, in order to obtain a continuous action of the lamp; it is necessary, however, to join the one carbon to the succeeding one, by drilling the ends and introducing a short piece of steel connecting-wire between the two. The metallic connection between the carbon and the carbon-holder is effected by contact springs and levers, the latter of which may be so arranged that, when through some mistake carbons have not been supplied to a lamp at the proper time, the contact lever in tipping up, short-circuits the working current, causing the extinguishment of the lamp, without stopping the working of other lamps within the same electrical circuit.

Further experiments have shown that the colour of flowers and

fruit subjected to continued electric light is much intensified. We have been able to bring strawberries to ripeness by means of the electric light fully a fortnight before the usual time, such fruit being remarkable for its colour and aromatic flavour. But it seems that the formation of sugar is not dependent upon this continuous light, and I might almost suggest the idea that the formation of sugar is the very last action that goes on in fruit: after the fruit has formed, developed, and acquired its aromatic qualities, then the formation of sugar seems to step in, as though it were the first stage of decay. Several botanists of high standing (Professor Cohen, of Leipzig, and others) have expressed the opinion that the growth of plants takes place chiefly at night; and there seems to be no doubt that during the night delicate and quick-growing plants, such as cucumbers and melons, make very considerable progress. But in that case they remain thin and yellow, whereas with continuous light they make less progress in length, but develop more in colour, in breadth, and in vigour; so that the truth may lie between the two views—that, for most rapid growth, intermittent light and darkness may be necessary, but that for vigorous development, and for the formation of fruit, it is not desirable.

The experiments now in preparation will perhaps settle some of these points, and will further show what can be done in this direction from a practical point of view.

Is it possible to make use of electric aid for growing plants and developing fruit that could be brought to the market? This is a mixed question. It depends, in the first place, upon the amount of effect that can be produced, and, in the second place, upon the cost. My opinion is, that the result of experiments on a large scale will come out favourably as regards its application to high-class horticulture. By working a steam engine, and using the waste steam to heat the water that circulates through the stoves, I believe that there will be very little extra consumption of fuel; and if that view be realised, the expense of the electric light will not be great at all, and the steam engine could be used in the daytime for various other ordinary useful purposes.

Then with regard to the spectrum experiments. It is an open

question which portion of solar light is really efficacious in forming chlorophyll, and which in promoting growth and in producing starch and fibrous matter. The difficulty in experimenting with solar light is obvious. Since the days of Joshua the sun has not been standing still, and the power of making it do so is beyond the skill of botanists, hence the difficulty of obtaining a standing spectrum by which to notice sensible effects produced from any portion of it. In this respect the electric light has every advantage, for, by placing an electric lamp in focus to produce a permanent spectrum, series of plants can be placed in different portions of it, and the various results noted for any required period.

## ON THE APPLICATION OF THE DYNAMO-ELECTRIC CURRENT TO LOCOMOTION.

I have frequently before this taken occasion to refer to the electric transmission of mechanical energy, and it is not my intention to revert to this subject generally, but to confine myself to an application for the propulsion of carriages along a railway, which has recently been carried into effect by my brother, Dr. Werner Siemens.

On the occasion of a local exhibition held in Berlin a year ago, a narrow gauge railway was laid down in a circle 900 yards long. Upon this railway a train of 3 or 4 carriages was placed, and upon the first carriage a medium-sized dynamo-electric machine so fixed and connected with the axle of one pair of wheels as to give motion to the same. The two rails, being laid upon wooden sleepers, were sufficiently insulated to serve for electric conductors. Between the two rails a bar of iron was fixed on wooden supports, through which the current was conveyed to the train by means of metallic brushes fixed to the driving carriage, while the return circuit was completed through the rails themselves. At the station the centre bar and two rails were connected electrically with the poles of a dynamo-electric machine similar in every way to the machine on the carriage, and which received motion from one of the engines on the ground. (A diagram was exhibited and explained, showing the arrangement, Dr. Siemens saying that he

was indebted to Mr. Shoolbred for it, who had prepared it for his own purposes.)

Between twenty and thirty persons could be accommodated on the carriages composing this train, the conductor riding on the first carriage, to which the form of a small locomotive engine has been given. Instead of the steam valve used in the latter, this engine is fitted with a commutator, by moving which the stopping, starting, and reversing of the engine can be effected.

It is a remarkable circumstance in favour of the electric transmission of power, that while the motion of the electro-magnetic or power receiving machine is small, its potential of force is at its maximum, and it is owing to this favourable circumstance that the electric train starts with a remarkable degree of energy. With the increase of motion the accelerating power diminishes until it comes to zero, when the velocity of the magneto or driven machine becomes equal to that of the dynamo or current-producing machine. Between the two limits of rest and maximum velocity the driving power regulates itself according to the velocity of the train; thus, on an ascending gradient the speed of the train diminishes, but the same effect is automatically produced which results from the turning on of more steam in the case of the locomotive engine. When running on the level, the velocity of the train should be such that the magneto-electric machine should make one-half to two-thirds as many revolutions per minute as the dynamo-electric. When descending, the speed of the magneto-electric machine will be increased, in consequence of the increased velocity of the train, until it exceeds that of the dynamo-electric machine, from which moment the functions of the two machines will be reversed; the machine on the train will become a current generator, and pay back, as it were, its spare power into store, performing at the same time the useful action of a brake in checking further increase in the velocity of the train. If two trains should be placed upon the same pair of rails, the one moving upon an ascending portion, the other upon a descending portion of the same, power will be transmitted through the rails from the latter to the former, which may therefore be considered as connected by means of an invisible rope.

The effects obtained with dynamo-electric machines under varying circumstances of load and velocity have been very fully investigated and brought forward by Dr. Hopkinson, F.R.S., in two papers read by him recently before the Institution of Mechanical Engineers, so that it would be superfluous for me to dwell upon this portion of the subject on the present occasion. Suffice it to say, that in transmitting the power of a stationary engine to a running train, the proportion of power actually transmitted varies with the resistance to, or speed of the train, reaching practically a maximum when the velocity of the machine on the train is about equal to two-thirds that of the current-generating machine, at which time more than fifty per cent. of the power of the stationary engine is actually utilised.

This little railway has been in operation daily for several months, affording great amusement to the visitors at the Exhibition. The magneto-electric engine exerts 5 horse-power, and it travels at a velocity of 15 to 20 miles an hour. Crowded trains left the station every five or ten minutes; and the pennies paid for the privilege of a seat have produced a considerable sum for the benefit of charitable institutions. Many who were not so fortunate as to secure a seat on the train amused themselves by touching the centre bar and one of the two rails after the train had passed, when a succession of electric discharges was distinctly felt.

The success attending this toy railway has given rise to the idea of useful applications upon a larger scale. An elevated tramway to connect one end of the city of Berlin with the other has been projected, but its execution has hitherto been delayed in consequence of the objections raised by the inhabitants of the streets through which the tramway was to pass. These objections would not apply, however, in many cases; and I have little doubt that before long we shall have electric tramways in connection with our mines, and for the conveyance of passengers along the roads between populous centres.

In passing through an adit or tunnel, the entire freedom of smoke from the electro-motor is a matter of great importance; and the administration of the St. Gothard Tunnel contemplate seriously the application of an electro-motor for conveying trains through

hat gigantic tunnel. Circumstances are in this case highly favourable to the employment of an electro-motor, because at both ends of the tunnel turbines of enormous aggregate power are actually established (having been employed in the operation of boring the tunnel), and all that has to be done is to insulate one of the rails, and to connect dynamo machines of sufficient power to the turbines and to the train itself.

Instead of the central bar, a copper or other conducting rope may be used to convey the current from the dynamo machine to the train. This conducting rope would rest upon wooden or glass supports, to be picked up by the train in order to pass over one or more contact pulleys, and to be again deposited behind the train. The central rail or copper conductor may, however, be entirely dispensed with if the two rails laid upon wooden sleepers are connected the one with the positive and the other with the negative pole of the dynamo machine. In this case care must be taken to insulate the wheels on one side of the train from those on the other, an object that can be attained by the adoption of wheels with wooden centres, and the metallic tires of the wheels on the one side must be put into metallic connection with the one pole, and the other with the other pole of the machine or machines on the train. Practice alone can determine which of these modes of construction is the best, but each can be made efficacious, and the preference will be due to economical or structural considerations.

The length of this paper has already exceeded, I fear, reasonable limits, or I might be tempted to enlarge upon the subject of the electric transmission of power. Enough has been said, however, to illustrate some of the uses to which this new form of energy may be rendered available for the purposes of man.

(At the close of the paper a dynamo machine was set to work, and supplied motive power to a circular saw, which cut up several pieces of timber from two to three inches square.)

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The PRESIDENT: It was said a very long time ago that "Hope deferred maketh the heart sick," but I am quite sure that our deferred hope has not produced this effect. On the other hand our hearts have been gladdened, and the warm applause with which

his paper has been received must have caused an excitement in the heart of Dr. Siemens. At least, let us hope it has.

On a previous occasion Dr. Siemens referred to the present age as essentially the "electric age." We have seen to-night how electricity has been applied to rival the sun, not only in assisting the growth of plants, but in producing temperatures that were supposed to be only confined to the surface of the sun. Dr. Siemens has outrivalled the famous Indian trick of producing a plant before our eyes, thus exceeding the dreams of any Arabian Nights' worthy; and, to paraphrase what was said about another thing, he has shown that the electric arc has a potentiality of power far beyond the dreams of engineers and workers in science.

The Romans and Greeks divided the history of the world into ages. They gave the name of the golden age to the time when all was supposed to be peace and happiness; then came the silver, brazen, and lastly the iron age, when all was misery; and this principle of dividing the world's history into ages was based upon the degeneracy of mankind. But the ancients were wrong. We have seen that by the conquest of civilisation over barbarism, and by the victory of science and enlightenment over ignorance, the world has greatly improved, and, instead of calling the first age the golden age, it really ought to be the name of the last—in point of fact, the one that should bear that name is the present electrical age of Dr. Siemens.

In days of old, power was represented by the Greek sculptors, sometimes by our old friend Jupiter throwing his thunderbolts in various directions; Vulcan was depicted as a man of massive arm hammering upon red-hot iron; speed, thought, and transmission of power were shown by Mercury; and the vivifying rays of the sun were typified by Phœbus Apollo. I would suggest that, if the Society in its beneficence desires to add another statue or bust to adorn its library, and should fix upon Dr. Siemens as their subject, they should commission a sculptor to produce a conglomeration of all the typifications I have enumerated, with a touch of Joshua to give respectability, as a fit representation of our worthy Past-President, whose name is so indelibly associated with the practical applications of electricity.

It is my pleasure and duty to propose a vote of thanks to Dr. Siemens for the very able paper he has given us, and also for the verbal descriptions of the experiments, and I am sure he has enabled us to go away with enlarged knowledge and grateful thanks to him for what he has done for us this evening.

Mr. LATIMER CLARK: Except as a matter of form and custom, it is quite unnecessary for any one to second the vote of thanks which has been proposed by our President. I am sure there can be but one desire in the mind of each of us present, and that is to return our deepest thanks to Dr. Siemens for the intellectual treat which he has just given us.

Electricity, a subject in which we are all so deeply interested, attracted general attention a long time ago, and was most diligently investigated, but for fully a hundred years no important use was made of it. By slow degrees, however, the electric telegraph came into existence, and we all know the giant proportions to which this has now attained. Then another long interval of time elapsed without anything of great practical importance being discovered; but now we seem again to be on the threshold of a whole galaxy of great discoveries and applications. We have seen three of them this evening. We have been enabled to realise the possibility, or rather the probability, that the electric current may be very largely and economically used, not only for the fusion of the more refractory metals and for chemical purposes, but also for large metallurgical operations. It is almost certain that when the sculptor to whom our President has alluded is called upon to cast the statue of Dr. Siemens, he will fuse his metal by the electric current.

We have seen the successful application of electricity to the uses of horticulture and growth of plants. Every one who knew that chlorophyll was formed by the influence of the actinic rays of light, and who knew that those rays were abundantly formed in the electric arc, must have foreseen that the day would come when experiments of this kind would be performed; but we owe especial thanks to Dr. Siemens, who possesses in such abundance the appliances and facilities necessary for these investigations, for the trouble and expense he has taken in making these experiments,

and in bringing before us such interesting results. He has proved that the sleep of plants, which was at one time thought as essential to them as it is to the animal world, is not a necessity; but that, on the contrary, plants can grow on continuously and without rest, increasing in health and strength. I heartily wish we could do the same.

The last subject he has so eloquently brought under our notice is the transmission of power by electricity and its numerous applications, and especially its adaptation to railway purposes. The future importance of these applications, and the extent of their possible development, it is difficult to over-estimate. But, as I have said, we all feel that we are on the verge of many new and wonderful applications of electricity, and that for their introduction and development the world will always be very largely indebted to our eminent Past-President. Let us then accord to him our sincere and earnest thanks for the pleasure he has afforded us this evening, and for the many and important services he has rendered to the science of electricity.

Dr. SIEMENS: Mr. President, Mr. L. Clark, my Lords, and gentlemen.—I do not know how to express my heartfelt thanks to you for the cordial manner in which you have proposed and accorded me your thanks. If I have succeeded in bringing before you some experiments that will convince any one who may have been sceptical or ignorant of the fact that we are on the verge of great achievements in a new direction, I hope you will be induced to look with interest on them as having been carried out with the wish and determination to aid each one of you, as members of the Society, in the great work before you, be that work for the weal of telegraphy in all its ramifications; be it in the development of the application of electricity for the decomposition of solutions, or the deposit of metals; be it for any of those more recent branches, some of which I have just had the honour and pleasure of bringing before you. I hope my labour has contributed to the result of convincing you that we have not yet completed our task as Telegraph Engineers and Electricians, but are rather at the beginning of a great career.

Be assured that I am highly sensible of the manner in which you have received my communication.

The PRESIDENT: I have three announcements to make, each of a different character.

Firstly—I call your attention to the piece of plate on the table. It is a testimonial, intended for presentation by his staff, to one whom many of you know. On the occasion of the recent marriage of Mr. J. Sivewright, the General Manager of Telegraphs at the Cape, his staff, to express the esteem in which they held their chief, subscribed together to make him a present, and commissioned me to carry out their idea. The piece of plate is the result. It is a work of art wrought by Messrs. Elkington, in silver, from a design given to them as being appropriate, and represents a telegraph pole surrounded by tropical plants, supported by an European and Caffre lineman, and a grouping of all the articles requisite in the construction of a line of telegraph.

Secondly—I again remind you that the Ronalds' Catalogue is in process of issue to subscribers, and the Librarian (Mr. Frost) will receive names of intending subscribers after the meeting. It is a book that every telegraph engineer should possess.

Thirdly—One of our most honoured members, a member of our Council, Professor D. E. Hughes, has to-day been elected a Fellow of the Royal Society.

A ballot took place, at which the following were elected, and the meeting adjourned till Wednesday evening, November 10th, 1880.

*As Associates:*

Arthur Edmund Baines.
James Brockie.
Féri Lovey.
John Alexander Lund.
Henry Upton.

# ORIGINAL COMMUNICATIONS.

## THE PRACTICAL MANAGEMENT OF SIR WILLIAM THOMSON'S TRAY BATTERIES.

By J. C. Cuff, Associate.

These batteries, which have been declared the worst feature of the recorder and the bane of the existence of those who have to attend to them, will be found to work in a delightfully satisfactory manner with a little proper attention of the simplest kind. No galvanometer or electrical testing, as is so frequently indulged in, is of the slightest necessity or even use as a part of the *regular* office routine. The batteries were introduced here with a regular system of testing, but this was found merely to show that a few cells, out of the 80 or more tested, were not up to the mark. The examination that followed this discovery generally revealed an absence of sulphate of copper in the cell, a supply of which instantly restored it to proper order. But if this failed, it was merely because the cell was short-circuited internally by deposited copper. Then a wire passed under the zinc, with its two ends projecting out of the cell and dragged backwards and forwards, would break away these connections and make all right again. If the badness of the cell proceeded from neither of these causes, then it was simply old age,—a good old age of twelve months or so,—and the only cure was to take it down and put it up anew. But we might know and do all this much quicker without the galvanometer than with it. The sundry other abnormal conditions of the cells that might arise from bad management I need not refer to now; for the object of this paper is merely to show how these important adjuncts of the recorder can be made to work perfectly with the least amount of trouble.

The merest inspection will show us whether any cell requires

sulphate of copper, and thus the first and most frequent fault is detected. But if we notice that the liquor in any cell becomes white much sooner than the others, then the second fault is detected and can be cured by dragging. But the cell must be watched for a few days to see whether the dragging has been effectual, and if, as rarely happens, it continues to get white too soon, it must be short-circuited externally. The third fault is also easily detected under the system followed out in Singapore, etc., because the piles of cells continue in good order for twelve months or more until they collapse.

The greatest part of the trouble in connection with the recorder formerly being that of keeping the batteries in order, they should be arranged in as convenient a way as possible. Now, if the lowest cell be only 4 or 5 inches from the ground, it is awkward to stoop down to attend to it properly; therefore, it is better to have a stand made about 18 inches high, 28 inches wide, and as long as the space will permit, or as the batteries to be accommodated may require, allowing 28 inches in length along this stand for each pile of the cells whose zinc plates are 16 inches square, which is the usual size. Above the battery stands are iron rods, on which slide stout curtains to keep out the dust and to prevent evaporation. In our battery room are three such stands 16 feet long, to take seven piles each. On these stands are fixed 2½-inch cubes of wood in proper positions for the corners of the trays to rest. Above each wooden cube is put a 2·5-inch square piece of vulcanite, and above that a piece of thin copper 5 inches square, with its sides bent slanting downwards—just like the old gutta percha umbrellas which would not stand the climate and have therefore given way to this much better form of insulator. The copper umbrellas are dipped in paraffin wax, to prevent crystals, etc., adhering, and their object is to keep the vulcanite dry and clean. Above these insulators the piles are erected: six cells in the first pile; in the second pile four cells, and then four of these wood-vulcanite-copper insulators, one on each of the four corners of the fourth zinc, and above these two more cells complete the pile. The third pile has six cells, the fourth is divided into four cells and two cells, and so on alternately.

This regular division of the batteries into two, four, and six cells is very convenient, because when the mousemills are at their best adjustment and the batteries as well, then four cells are ample, but usually six cells are required, and sometimes it is necessary to add two more for our work.

From each of these sets of cells a pair of electrodes is led away to a terminal box in the instrument-room, and in juxtaposition to these battery terminals are others from which wires are led away to the several recorders. Thus there are sixty terminals for the batteries and twenty-four for the six recorders; being two for the signal coil electro-magnets, and two for the mousemill of each instrument. This terminal box is of considerable advantage in the working of the recorder, not only because it prevents all that distressing confusion which those who had charge used to get into, in tracing out where the wires went, but also because it so greatly facilitates any changes in the arrangement of the batteries. A peculiar instance of this convenience is the following. An occasion sometimes arises when, through cells being in process ot renewal, or of removal, or a greater demand than usual, it is very advantageous, just for the emergency, to join up the same set of batteries on two or three recorders. On account of the very low resistance of the battery, very nearly the same strength of current is got in each of the two or three instruments, thus joined up to the same battery in multiple arc, as was previously got in one only.

The resistance of the large electro-magnets being about 16 ohms, the resistance of the twenty-tray cells used thereon being about 2 ohms, and calling the E.M.F. 20

$$\text{we have } Q=\frac{E}{B+R}=\frac{20}{2+16}=1{\cdot}1$$

whilst in the second case, with 2 recorders in multiple arc on same battery, we have only 8 ohms external resistance, which gives

$$\frac{E}{B+R}=\frac{20}{2+8}=2$$

as the strength of current going out from the battery, which, being halved in each instrument, gives us a working strength of current equal to 1 as against 1·1 in the former instance.

It is better to couple two mousemills or two pairs of the large

electro-magnets than to put the large electro-magnets and the mousemill of one recorder on the same battery. In the latter case, if more than six cells be used, it will be necessary to insert resistance in the mousemill circuit of 50 ohms or so, which can be adjusted to make the mousemill go at any speed desired. The great control thus obtained over the speed of the mousemill makes it a very favourite arrangement; but it was found liable to be sadly abused, it being so easy to insert a few plugs so as to cut out resistance and make the mill go faster, that this was constantly done instead of cleaning and adjusting the contact breaker at the back.

Two other very nice arrangements of the recorder were found in practice to be so often abused that we had to abandon them, viz., the spring marked $Y_3$, and the terminal marked $U_3$ on page 188 of Vol. V. of this Society's Journal. The former puts the large electro-magnets in multiple arc, so that the resistance is reduced from 16 ohms to 4, and thus the supply of sulphate of copper in the batteries, which should have lasted for two days, was used up in half-a-day, and much trouble was frequently the result. But the great objection to all these multiple arc arrangements is the bad pulverulent deposit that occurs in the cells when they are working through too low an external resistance. To prevent this, prohibition and instruction availed nothing, and we had to remove the offending spring altogether. After this, the time the batteries would work without requiring more sulphate of copper became definite and much more satisfactory.

The intermediate terminal $U_3$ we no longer join up, because it either involves the use of an additional wire or the fork from one of the mousemill wires, which, though simple enough, seems to be considered a very confusing arrangement, and in return for which we only get, in practice, the ability to put *less* power on the electro-magnets, because the men using the instrument will always persist in putting the switch on full, in order to have the signals yet more clear if possible.

Returning to the subject of setting up the tray batteries, the directions given on page 194 of Vol. V. involve much unnecessary trouble. Thus, if the batteries be attended to in the simple way

now indicated, it will be found that the parchment paper is worse than useless. The zinc gratings will be found to keep hard and clean to the end of the twelve months or so, and there will be no dirt falling away from them for the paper to arrest, as formerly. On the other hand, the paper facilitates the formation of threads of copper, which short-circuit the cell internally. In dragging a wire across to rupture these connections in the way before described, the paper is torn to bits, and its further uselessness evident. Again, it is quite unnecessary to varnish the under-surface of the thin copper plate.

The strip of copper which is soldered to the bottom of each tray is to be cleaned bright after the varnish has set hard and then to be threaded through two slits made in the thin sheet of copper with a penknife, after which all is to be rubbed smooth with a stoneware block. This connection will be found easier, neater, and more reliable.

The stout copper wire which is soldered on the corner of each tray is made about 4 inches long, and soldered on by its middle. This is no more trouble, and gives two ends to make connection, instead of one, which is often very convenient.

The stand, with its wood-vulcanite-copper insulators, being arranged as before described, the first tray is placed in position thereon, and levelled with a common spirit-level attached to, or, still better, let into, a bar of wood 16 inches long. This is better in general than pouring in water, because the piles are usually put up in readiness some time before they are required, and they keep in better order if they are left quite dry until the day it is desired to start them. The first tray and the first zinc are levelled, after which the zincs only are levelled, by inserting thin scraps of wood (cut the size and split for the purpose) under the corners as required. The trays will come quite right in this way, and it will be done quicker. No bits of wood should be put under the trays, because each of these should make good clean contact with the zinc beneath. Also observe that the grooves under the trays and the zincs are all facing in that way and direction which is the most convenient for inserting the funnel for filling them.

The piles should thus be put up dry and kept dry until they

are wanted, when they should be filled up with a clear white solution of sulphate of zinc, about one-quarter saturated, or 24 on Twaddell's scale. Then half-a-pound of sulphate of copper is put into each tray, distributed round the four sides, and the battery at once joined up to a circuit of about 2 or 3 ohms per cell, and left working undisturbed for at least 24 hours, in order that a good firm deposit of copper may be thrown down. If care is taken in starting the batteries, they are so much more likely to work well; whereas if they are set going on short-circuit or through too low a resistance for the number of cells, the deposit in the battery will be of a dirty pulverulent form.

A portion of the liquor drawn off from the old cells is preserved in some large stoneware jars which contain about 40 gallons each. Scraps of old zinc are kept suspended in these to precipitate the copper if the liquor be at all blue. When it has become quite colourless it is ready for use, after being diluted with water to the required density. In the case of starting these batteries at a station for the first time, where there is no old liquor and no sulphate of zinc to mix up, it will be better to sift the dust out of the sulphate of copper from the larger crystals, and dissolve it in water, when the sulphate of zinc solution may be produced by suspending any scraps of old zinc and stirring occasionally. This same solution is the best for starting Minotto cells, because they are then ready for use at once without requiring the usual destructive short-circuiting and delay.

The batteries having been started properly, the two chief things required to keep them going well are, to supply them regularly with half-a-pound of sulphate of copper as often as the liquor gets of a faint blue colour, and once-a-week regularly to draw off a portion of the liquor and dilute it to the proper density. The large crystals of sulphate of copper must be broken smaller, the dust must be sifted out of it, and a definite quantity always weighed for each cell,—about half-a-pound is most convenient,—but on no account should a fresh supply be put in until the old be nearly exhausted.

Regularly every Saturday ... d by the hydrometer and then di ... s is

most easily accomplished by the use of the following table, which shows the exact amount to remove and replace by clean water for any reading on the hydrometer.

Each tray cell, 22-inch, contains 26 lbs. of water, 2·6 gallons, or 20·8 pints. The G. P. Minotto cell used as measure contains 28 fluid oz., or 1·4 pint. Column 2 is from "Storer's Dictionary of Solubilities." From these data the following table is constructed, in which—

Column 1 shows the hydrometer reading,
,, 2 the percentage of sulphate of zinc in solution,
,, 3 amount of sulphate of zinc in each tray,
,, 4 surplus sulphate of zinc,
,, 5 amount of liquor to be removed and replaced by clean water,
,, 6 the same as column 5, but the figures reduced to suit the Minotto cell, which is used as measure.

| 1 | 2 | 3 | 4 | 5 | 6 |
|---|---|---|---|---|---|
| | P.C. | LBS. | LBS. | PINTS. | |
| 24 | 20·00 | 5·2 | 0 | 0 | 0 |
| 26 | 21·43 | 5·6 | ·4 | 1·5 | 1·1 |
| 28 | 22·86 | 5·9 | ·7 | 2·5 | 1·8 |
| 30 | 24·29 | 6·3 | 1·1 | 3·6 | 2·6 |
| 32 | 25·57 | 6·6 | 1·4 | 4·4 | 3·1 |
| 34 | 27·14 | 7·1 | 1·8 | 5·3 | 3·8 |
| 36 | 28·57 | 7·4 | 2·2 | 6·2 | 4·4 |
| 38 | 30·00 | 7·8 | 2·6 | 6·9 | 4·9 |
| 40 | 31·57 | 8·2 | 3·0 | 7·6 | 5·4 |
| 42 | 32·71 | 8·5 | 3·3 | 8·1 | 5·8 |
| 44 | 34·20 | 8·9 | 3·7 | 8·6 | 6·2 |
| 46 | 35·57 | 9·3 | 4·1 | 9·2 | 6·6 |
| 48 | 37·14 | 9·7 | 4·5 | 9·6 | 7·0 |

In practice, only the first and last columns are written on a piece of stout cardboard. This the battery-man refers to every time, until after a while he remembers it accurately, and is therefore able to dispense with the written table.

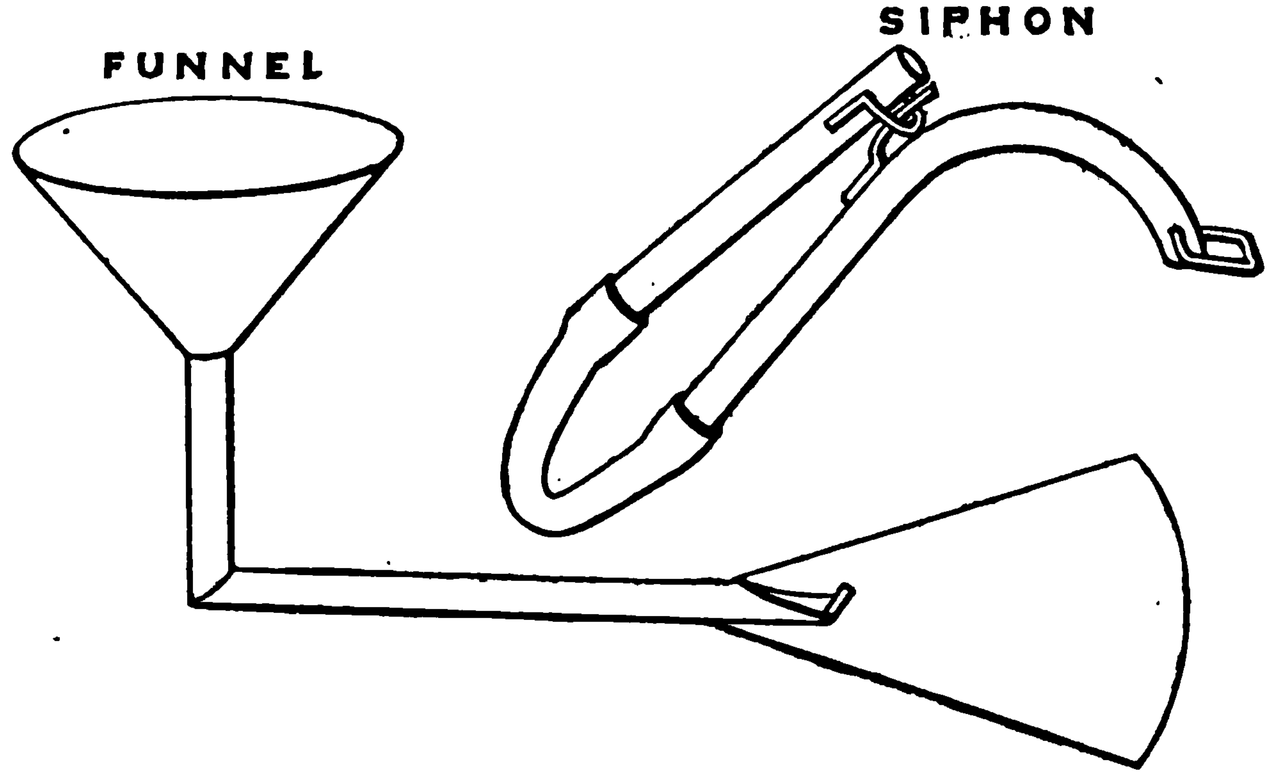
This table will be found to give exact results, provided the following precautions are observed:—

1st. Pour clean water over the zincs, until the level is about a quarter-of-an-inch above the grating, before testing for specific gravity.

2nd. Always keep the end of the siphon at the same level in the liquor, just below the zinc. This is ensured by using the siphon with gauge attached, which must be provided for the purpose.

3rd. Draw off some of the liquor in this way, and test its specific gravity. Then draw off the quantity indicated in the table opposite to the hydrometer reading, and pour in the same amount of clean water over the zinc. More than two G.P. measures should not be removed at once, or the level will be too much lowered. The same amount of pure water being replaced, some more may be drawn off from the cell until the total amount so drawn off, including that used for testing the specific gravity, shall be equal to that indicated in the table.

The specific gravity of the liquor in the cell at that level will be found to equal 24 on the hydrometer scale. This scale, known as Twaddell's, does not give the specific gravity direct. This, however, may be at once read off by dividing the scale reading by 200, and adding 1 to the quotient. Thus 24 scale reading, divided by 200, gives 0·12. To this add 1, and we get 1·12 as the specific gravity when water is taken as unity.




Two articles of considerable utility in managing the batteries are the funnel and the siphon. The former is made of tin or

copper, with the lower portion of the tube at right angles to the upper, and flattened at the end. Thus it can be inserted horizontally above the zinc, and disturb the liquor as little as possible. This object may be still further attained by soldering a fan-shaped flat piece of metal under the nozzle horizontally, and projecting from it about five inches.

The siphon used is very simple and most convenient. A piece of $\frac{3}{8}$ compo. gas pipe, about 10 inches long, is bent into a siphon shape by a gentle curve, with one one arm much longer than the other. To the end of the short arm is soldered a square loop of wire, bent into an L shape, so that it can hook under the zinc, and always draw off the liquor from a definite stratum. To the other end is attached a piece of india rubber tubing about four inches long, and this, in its turn, to another straight piece of compo. pipe. To the end of this pipe is soldered a loop of wire, which can be slipped on to a catch, which is soldered on the first-mentioned piece of compo. pipe, near the top of the straight portion of its long arm. The india rubber forms a hinge, and thus the short piece of pipe being either hooked up or let down, acts as a tap. This siphon being once charged, it can be moved from cell to cell, and the liquor flows whenever required, without its having to be recharged, drawn, or held continually. The sketch will make clear the construction of these two simple but most convenient and important articles.

When the batteries have been working for about twelve months, the piles subside somewhat and lean over a little. They must then be taken down, and, if they have been kept carefully, there will be very little loose dirt found therein. The zinc gratings will be compact, and of the original shape and size, but very brittle, like soft slate pencil, with a thin vein of zinc in the centre, less than the tenth of an inch in diameter.

If the trays are turned upside down, one edge raised about six inches and let drop, the copper, not having been varnished, will easily detach itself from the tray, and can be removed in a solid slab of valuable metal, weighing about 20 or 30 lbs. All this deposit should be preserved and sold, and thereby a considerable diminution effected in the expense of maintaining these batteries.

SINGAPORE, 31*st March*, 1880.

KURRACHEE, *24th April*, 1880.

*To the Secretary of the Society of*
*Telegraph Engineers, London.*

Sir,—In August last I forwarded to the Society an account of some remarkable phosphorescence in the Persian Gulf, witnessed by me on the 15th May. My attention has since been called to a letter from Commander Pringle of H.M.S. *Vulture*, which appeared in "Nature," page 291, July 24th, a copy of which I enclose for your information.

The independent testimony of an observer who witnessed the same phenomenon at the same time, but at a distance of 180 miles to the west of where it was seen by myself and others, on board the *Amberwitch* on the 15th May, is so important that I think it is well worthy of record in the Journal of the Society.

HENRY C. MANCE.

(From "Nature," July 24, 1879.)

## REPORT OF AN UNUSUAL PHENOMENON OBSERVED AT SEA.

The following Report to the Admiralty has been communicated to us for publication by Capt. Evans, C.B., F.R.S., the Hydrographer to the Navy:—

"H.M.S. *Vulture*, BAHREIN, May 17, 1879.

"SIR,—I have the honour to inform you that, at about 9.40 p.m. on May 15th, when in lat. 26° 26′ N. and long. 53° 11′ E., a clear, unclouded, starlight night, Arcturus being within some 7° of zenith, and Venus about to set; wind north-west, force 3, sea smooth, with slight swell from the same direction; ship on starboard tack, heading west-south-west and going three knots, an unusual phenomenon was seen from the vessel.

"I noticed luminous waves or pulsations in the water, moving at great speed and passing under the ship from the south-south-west. On looking towards the east, the appearance was that of a revolving wheel with centre on that bearing, and whose spokes were illuminated, and looking towards the west a similar wheel appeared to be revolving, but in the opposite direction. I then went to the mizen top (fifty feet above water) with the first lieutenant, and saw that the luminous waves or pulsations were really travelling parallel

to each other, and that their apparently rotatory motion, as seen from the deck, was caused by their high speed and the greater angular motion of the nearer than the more remote part of the waves. The light of these waves looked homogeneous, and lighter, but not so sparkling, as phosphorescent appearances at sea usually are, and extended from the surface well under water; they lit up the white bottoms of the quarter-boats in passing. I judged them to be twenty-five feet broad, with dark intervals of about seventy-five between each, or 100 from crest to crest, and their period was seventy-four to seventy-five per minute, giving a speed roughly of eighty-four English miles an hour.

"From this height of fifty feet, looking with or against their direction, I could only distinguish six or seven waves; but, looking along them as they passed under the ship, the luminosity showed much further.

"The phenomenon was beautiful and striking, commencing at about 6h. 3m. Greenwich mean time, and lasting some thirty-five minutes. The direction from which the luminous waves travelled changed from south-south-west by degrees to south-east and to east. During the last five minutes concentric waves appeared to emanate from a spot about 200 yards east, and these meeting the parallel waves from south-east did not cross, but appeared to obliterate each other at the moving point of contact, and approached the ship, inclosing an angle about 90°. Soundings were taken in twenty-nine fathoms; Stiffe's Bank, with fifteen to twenty fathoms, being west about one mile. The barometer was already at 29·25 from 8 to 12 p.m.

| | At 8 p.m. | 10.15 p.m. | Midnight. |
|---|---|---|---|
| Temperature of air | ... 84 | ... 83 | ... 83 |
| Temperature of sea-water | 84 | ... 82 | ... 82 |

"I observed no kind of change in the wind, the swell, or in any part of the heavens, nor were the compasses disturbed. A bucket of water was drawn, but was unfortunately capsized before daylight. The ship passed through oily-looking fish spawn on the evening of the 15th and the morning of the 16th inst.—I have the honour to be, Sir, your obedient servant,

"J. ELIOT PRINGLE, Commander."

## IRON-ZINC CELLS.

SIR,—The satisfactory accounts given by Professor Hughes and Mr. Kempe of iron-zinc cells led to some twenty or more being put up here to work an electro-magnet. Finding, however, that the current produced by them fell off considerably, I was led to make the following experiments with different strengths of acid. In each cell the iron and zinc plates had 8¾ square inches of immersed surface opposed to one another. They were separated by 1¼ inches of liquid, and kept in their places rigidly by paraffined glass. The solutions employed were respectively—

| | | | | | | |
|---|---|---|---|---|---|---|
| 1 | of sulphuric | acid with | 30 of | water, by volume, | sp. gr. | 1·04 |
| 1 | ,, | ,, | 20 | ,, | ,, | 1·05 |
| 1 | ,, | ,, | 10 | ,, | ,, | 1·08 |
| 1 | ,, | ,, | 5 | ,, | ,, | — |

The electro-motive force of the cells was measured first with an electrometer and then with a tangent galvanometer, the resistance only with the latter. With the electrometer tests a Daniell cell was employed as a standard, and its electro-motive force taken as unity.

| Date 1880. Condition of Cells. | Electromotive Force. | | | | | | Internal Resistance | | |
|---|---|---|---|---|---|---|---|---|---|
| | Galvanometer. | | | Electrometer. | | | in Ohms. | | |
| | Cells. | | | Cells. | | | Cells. | | |
| | 1 to 30 | 1 to 20 | 1 to 10 | 1 to 30 | 1 to 20 | 1 to 10 | 1 to 30 | 1 to 20 | 1 to 10 |
| June 16th.—Just put up | 0·4 | 0·45 | 0·47 | 0·54 | 0·52 | 0·56 | 0·3 | 0·35 | 0·38 |
| After all three had been joined up in series for 1h. 20m. through a resistance of 10 ohms ... | 0·47 | 0·48 | 0·47 | 0·51 | 0·56 | 0·57 | 1·3 | 0·26 | 0·45 |
| June 17th. — After all three had been joined up in series for 17h. through a resistance of 10 ohms. ... ... ... | 0· | 0·16 | 0·5 | 0·01 | 0 27 | 0·53 | No deflection | 3·85 | 1·56 |
| June 18th. — After 24h. rest ... ... ... ... | 0·02 | 0·15 | 0·85 | 0·02 | 0·38 | 0·42 | 1·85 | 3·85 | 4·51 |

| Date 1880. Condition of Cells. | Electromotive Force. | | | | Internal Resistance. | |
|---|---|---|---|---|---|---|
| | Galvanometer. | | Electrometer. | | In Ohms. | |
| | Cells. | | Cells. | | Cells. | |
| | 1 to 10 | 1 to 5 | 1 to 10 | 1 to 5 | 1 to 10 | 1 to 5 |
| June 18th.—Just put up | 0·43 | 0·43 | 0·56 | 0·55 | 0·51 | 0·51 |
| June 18th.—After the two had been joined up in series for 1h. 20m. through a resistance of 10 ohms ... ... ... | 0·46 | 0·45 | 0·56 | 0·55 | 0·34 | 0·4 |
| June 19th.—After the two had been joined up in series for 48h. through a resistance of 10 ohms ... ... ... | 0· | 0·35 | 0·01 | 0·55 | No deflection | 2·85 |
| After several days' rest | ... | ... | ... | ... | about | 10 |

With the weak acid, then, the iron-zinc cell soon runs down so as to have no electro-motive force at all, and with the strong acid the resistance increases so much as to much enfeeble the current if the external resistance be at all small. The iron-zinc cell is therefore unsuitable for sending a constant current through a small external resistance. Some experiments were then made as to the effect of heating the iron plate, and it was found that the current sent through a small external resistance by a cell that had been in use for some days could be doubled by washing the iron plate. In fact, merely taking out the iron plate and exposing it to the air much increased the deflection.

To compare the behaviour of these cells with that of Daniell's cells similarly treated, four Daniell's cells were prepared. In all the copper sulphate solution was saturated, while in two the dilute sulphuric acid was in the proportion of 1 of acid to 20 of water by volume, while in the other two it was respectively in the proportion of 1 to 10 and 1 to 5. The well-known constancy of the Daniell cell came out prominently, for, on joining the four cells in series and causing them to send a current through 10 ohms for 1h. 20m. on the 29th June, again for 5h. 30m. on the 30th, and

lastly for 20h. between the 30th and the 1st July, the internal resistances, which were respectively 0·96, 1·33, 0·6, 1·08 ohms soon after the cells were put up on the 29th had become 1·65, 1·37, 1·16, 0·73, on the 1st July, the electro-motive forces in the case of the first three cells had apparently even slightly increased, and in the fourth remained almost constant. The last Daniell cell, in which the dilute acid was in the proportion of 1 to 5, gave the best results, and was far more constant and effective than any of the iron-zinc cells.—I remain, &c.,

F. J. MUDFORD.

The Physical Laboratory,
City and Guilds of London Institute,
Cowper Street.

# ABSTRACTS.

## HÄNEKE—THE MEASUREMENT OF BATTERY RESISTANCES.

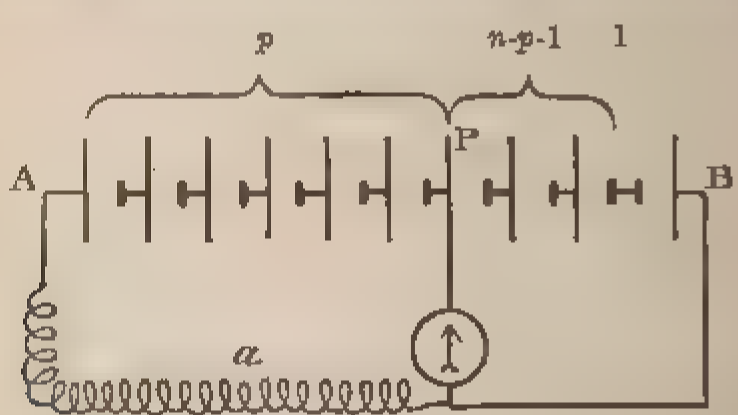
(*Electrotechnische Zeitschrift, H. IV., April*, 1880, *pp.* 134–5.)

In determining the proper arrangements to give to any battery, we require to know merely the ratio of the resistance of each cell to that of the external resistance, independently of their actual resistances; for if there are $s$ cells in series and $m$ in multiple circuit, if $b$ is the internal resistance of each of the $n$ cells and $a$ is the external resistance, then $m$ and $s$ ought (as is well known) to satisfy the equations

$$m = \sqrt{\frac{n\,b}{a}}$$

$$s = \sqrt{\frac{n\,a}{b}}$$

and can therefore at once be found if $\frac{b}{a}$ is known. To ascertain this ratio, join up the battery with ($n$—1) cells in series and the $n$'th cell opposed to these, so that the electromotive force of the arrangement is that of ($n$—1) cells. With this battery send a current through the given external resistance $a$, and find by experiment a point P in the battery, such that if it is joined through a galvanometer to the extreme end of the battery B on that side in which the single cell has been placed, no current flows. When such a point in the




battery has been found, let there be $p$ cells in the other side of the battery, or between A and P, then it can be shown that

$$\frac{b}{a} = \frac{n-(p+1)}{2\,p}.$$

If, however, we cannot find any point P giving no current through the galvanometer, we must find two adjacent binding screws in the battery at which we get plus and minus currents, then the above expression gives two limits for the value of $\frac{b}{a}$. The author gives a numerical example of his

method, the use of which is limited by $\frac{b}{a}$ being necessarily not less than $\frac{1}{2(n-3)}$, and not greater than $\frac{n-3}{2}$, $p$ must, therefore, lie between $(n-3)$ and 1: if, however, the method can be supplemented by the use of a rheostat or resistance box, then a more accurate answer is easily obtainable.

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## ZELLI—THE UNDERGROUND TELEGRAPH IN VIENNA.

(*Electrotechnische Zeitschrift, H. IV., April*, 1880, *pp*. 135-142.)

The cable, made by Messrs. Rattier & Co. of Paris, consists of a strand of seven copper wires 0·6 mm. in diameter, surrounded by a gutta percha covering 5 mm. external diameter, with a coating of tarred cotton-wool yarn. Seven of these cores form one cable, and protected with—1st, a tarred woollen ribbon; 2nd, a tarred hemp covering, dipped previously in a solution of sulphate of copper; 3rd, a second tarred woollen ribbon. Each core has a conductivity from 91 to 98 per cent. of that of pure copper, an insulation from 1100 to 3000 megohms per kilometre at 20° C., and a capacity 0·21 to 0·25 microfarads per kilometre. The weight of each core is, according to the contract, within 5 per cent. of 390 kilometres per kilometre. The tar was quite free from acid, but the hemp covering was slightly acid from being dipped in the copper sulphate solution. The cable was made in lengths of 500 metres, and laid at a depth of 1·3 metres in a box of red larch-wood, several such cables being stretched in the box, and separated from one another by a mixture of one part of Beocsimer cement and two parts of sand. Where the cable crossed the Franzensketten bridge, the wires, to avoid damage from the vibration, were not stretched tight, but laid simply in a layer of sifted coal ashes placed in the box.

A channel 0·8 m. wide and about 1·4 m. deep was dug for the wires to lie in, and the bottom made quite smooth; the depth, 1·4 m., being always followed except when gas pipes, &c., had to be avoided. The boxes in which the wires were laid were from 3 to 5 m. long.

The cable, containing seven distinct conductors, cost about £112 per kilometre, and the complete cost of making and laying 4,202 kilometres of subterranean line containing 33,431 kilometres of cable was about £5,500.

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## F. GERALDY—THE ANCESTRY OF THE JAMIN LAMP.

(*La Lumière Electrique T. II. No.* 12, *June* 15*th*, 1880, *pp*. 234-6.)

The important features in this lamp are:—1. *The employment of two parallel carbons.* 2. *When the current is not passing the carbons touch at their ends, and they are separated by the action of an electro-magnet excited by the current.* 3. *This electro-magnet is formed of a plate of iron placed saddle-wise on a conductor.* 4. *The current traverses the length of the carbons; the flame is fixed, and shortened by the action of a coil of wire in circuit with the lamp.* In the complete lamp, three sets

of carbons are combined in one framework. 5. *The current, led indifferently to all the candles, chooses for itself, and lights that one which it finds most suitable.* 6. *When one candle is burnt down, a spring cuts it out of the circuit, and compels the current to pass to another.* 7. *If by accident one light goes out, then by an automatic arrangement a resistance equal to that of the lamp is immediately substituted for it, so as not to vary the total resistance in circuit.* As regards No. 1—parallel carbons—this idea was proposed by W. E. Staite in 1846, and was employed by Werdermann for cutting rock in 1874,* who at the time proposed, in addition, to use an electro-magnet to make the arc pointed. As regards No. 2—separating the parallel carbons by an electro-magnet—this is the well-known system of Wilde No. 3—the saddle-shaped piece of iron—this was proposed by Pulvermacher about 1852, a complete account of which may be found in the Exposé des Applications de l'Electricité, by the Count du Moncel, p. 211, second edition of 1856. No. 4—fixing the arc by a current circulating in a coil of wire—as already mentioned, Werdermann in 1874 proposed employing an electro-magnet for this purpose, and in a separate article by M. Hospitalier, in the same number of *La Lumière Electrique* it is shown that the arrangement of M. Jamin would be disadvantageous rather than otherwise. No. 5—the current selecting the candle—such an arrangement was employed for a long time by the Société Générale with the Jablochkoff candle, and finally abandoned, owing to the great inconvenience it caused. No. 6—cutting a candle out of the circuit—when using the Gadot burner, the Société Générale had to employ a device like that proposed by M Jamin, depending on the burning of a wire and the liberation of a spring; like the previous arrangement also, the Société Générale have abandoned it as being unsuccessful. No. 7—the introduction of an equivalent resistance on the extinction of a candle—M. Jamin has not described how he proposes to attain this result, but Messrs. Siemens, Werdermann, Edison, De Mersanne, and Reynier have already described methods for fulfilling this object.

## C. H.—TESTS OF BROOKS' CABLE.

(*La Lumière Electrique, T. II. No.* 12, *June* 15*th*, 1880, *p.* 241.)

Tests made May 14th, 1880, by M. Aylmer, on a specimen of Brooks' underground cable laid last November at Versailles.

| No. of the Conductor. | Insulation resistance in megohms per kilometre. | Capacity in microfarads per kilometre. |
|---|---|---|
| 1 | 178·5 | 0·106 |
| 2 | 238·0 | 0·087 |
| 3 | 223·1 | 0·087 |
| 4 | 238·0 | 0·087 |
| 5 | 198·3 | 0·106 |
| 6 | 198·3 | 0·106 |
| 7 | 198·3 | 0·106 |

* "Some Historical Notes on the Electric Light," Col. Bolton, *Jour. Soc. Tel. Eng.*, Vol. VIII., No. 27, p. 259.

| No. of the Conductor. | | Insulation resistance in megohms per kilometre. | | Capacity in microfarads per kilometre. |
|---|---|---|---|---|
| 8 | ... | 198·3 | ... | 0·106 |
| 9 | ... | 223·1 | ... | 0·109 |
| 10 | ... | 223·1 | ... | 0·109 |
| 11 | ... | 223·1 | ... | 0·109 |
| 12 | ... | 223·1 | ... | 0·106 |
| 13 | ... | 223·1 | ... | 0·106 |
| 14 | ... | 210·0 | ... | 0·106 |
| 15 | ... | 223·1 | ... | 0·106 |
| 16 | ... | 238·0 | ... | 0·109 |
| 17 | ... | 223·1 | ... | 0·109 |
| 18 | ... | 223·1 | ... | 0·106 |
| 19 | ... | 223·1 | ... | 0·106 |
| 20 | ... | 223·1 | ... | 0·109 |
| 21 | ... | 223·1 | ... | 0·109 |
| 22 | ... | 223·1 | ... | 0·106 |

The time of electrification was five minutes. The tests repeated on each wire, all the other wires being put to earth, gave the same results.

Tests taken by Mr. Brooks, March 26th, 1880, gave—

| No. of Conductor. | | Insulation Resistance in megohms per kilometre. | | Time of electrification. |
|---|---|---|---|---|
| 1 | ... | 203 | ... | 5 |
| 22 | ... | 254 | ... | 5 |
| 12 | ... | 135 | ... | 1 |
| ... | ... | 193 | ... | 2 |
| ... | ... | 254 | ... | 5 |

Wire No. 12 was now again tested, all the other wires being to earth, and the following results were obtained, the temperature being about 26°·6 C.

| | | | | |
|---|---|---|---|---|
| 12 | ... | 176 | ... | 1 |
| ... | ... | 203 | ... | 2 |
| ... | ... | 226 | ... | 3 |
| ... | ... | 256 | ... | 4 |
| ... | ... | 256 | ... | 5 |

Other tests made at Brussels by the engineers of the Belgian administration gave—

| Date. | | Resistance in megohms per kilometre. | | Capacity in microfarads per kilometre. | | Temperature centigrade. |
|---|---|---|---|---|---|---|
| 11·9·79 | ... | 1787·9 | ... | 0·016 | ... | 18° |
| 14·9 | ... | 627·7 | ... | 0·016 | ... | 18° |
| 19·9 | ... | 464·8 | ... | 0·014 | ... | 18° |
| 28·9 | ... | 657·2 | ... | 0·013 | ... | 16° |
| 4·10 | ... | 531·1 | ... | 0·014 | ... | 16°½ |
| 11·10 | ... | 488·5 | ... | 0·013 | ... | 14° |
| 24·10 | ... | 439·5 | ... | 0·014 | ... | 11° |
| 7·11 | ... | 324·6 | ... | 0·013 | ... | 10° |

| Date. | | Resistance in megohms per kilometre. | | Capacity in microfarads per kilometre. | | Temperature centigrade |
|---|---|---|---|---|---|---|
| 14·11 | ... | 857·25 | ... | 0·012 | ... | 7° |
| 23·11 | ... | 1485 | ... | 0·012 | ... | 3° |
| 3·1·80 | ... | 1324·5 | ... | 0·012 | ... | 7° |
| 9·1 | ... | 1281 | ... | 0·011 | ... | 4° |
| 17·1 | ... | 1323 | ... | 0·011 | ... | 1° |
| 23·1 | ... | 2056·23 | ... | 0·012 | ... | ... |
| 28·4 | ... | 904·5 | ... | 0·018 | ... | ... |
| 14·5 | ... | 290 | ... | 0·011 | ... | 24° |
| 28·5 | ... | 279 | ... | 0·015 | . | ... |

## H. PELLAT—THE MEASUREMENT OF THE ELECTROMOTIVE FORCES OF BATTERIES, AND OF TWO METALS IN CONTACT.

(*Journal de Physique, T. IX. No.* 101, *May*, 1880, *pp.* 145, 152.)

The author first describes a method for subdividing an electromotive force analogous to that employed when using Thomson and Varley's sliding resistance coils. He then proposes to place two plates of different metals near one another, and instead of joining them by a simple metallic wire as usual when measuring the electromotive force, to join them by a wire in which is maintained a known and variable difference of potentials. This difference of potentials is varied until the difference of potentials between the plates themselves is nought, as evidenced by neither possessing, after separation, any charge as measured by Hankel's electrometer, which is a modification of Bohnenberger's. The difference of potentials, measured by a Lippmann's electrometer, necessary to be inserted to produce this balance gives directly the difference of potentials it is desired to know to the $\frac{1}{10000}$ of a Daniell for small electromotive forces, and to the $\frac{1}{5000}$ of a Latimer Clark's cell for larger. M. Pellat points out that the difference of potentials between two metals not touching but joined by a wire, depends on the insulating medium between them, but he says that his experiments show that the nature and pressure of the gaseous medium surrounding the two plates affects very slightly the observed difference of potentials. He also points out that the difference of potentials between two portions of the same metal in contact, but at slightly different temperatures, is far greater than the mean thermo-electric power for the same difference of temperatures.

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## MARCEL DESPREZ—THE EFFICIENCY OF ELECTROMOTORS, AND THE MEASUREMENT OF THE AMOUNT OF ENERGY IN A CIRCUIT.

(*Journal de Physique, T. IX. No.* 102, *June*, 1880, *pp.* 195, 9.)

Let I be the strength of the current in a circuit containing an electromotor when the motor is at rest and $i$ its value when the motor is allowed to run. Let $r_2$ be the resistance necessary to be inserted in the circuit to diminish the

current from I to $i$ when the motor is at rest, then if $r_1$ is the resistance of the rest of the circuit, the energy given out as heat will be $i^2 r_1$, and as available work, $i^2 r_2$. The efficiency of the electromotor will therefore be $\frac{r_2}{r_1 + r_2}$, which may be put in the form $\frac{E}{E_1}$, where E is the back electromotive force of the motor, and $E_1$ the electromotive force of the generator. To measure the energy in a circuit join two points with an auxiliary circuit of great resistance, in which is placed a high-resistance galvanometer, while in the main circuit is placed a low-resistance galvanometer, the energy in the main circuit will then be measured by the product of these two currents. The simplest method of obtaining this product is to suspend the low-resistance coil inside the high-resistance one, contact with the two ends of the movable coil being maintained by two wires dipping into mercury cups, and a special integrator, devised by the author in 1876, being attached to the apparatus will give the integral of the products of the two currents with respect to time.

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## J. L. HOORWEG—HEAT THEORY OF THE GALVANIC CURRENT.

(*Annalen der Physik und Chemie*, No 4, 1880, *B. IX.*, *H.* 4, 552-90.)

Clausius points out that Helmholtz's contact theory is inadequate, and he chose to elaborate the theory of the Dutch physicist, Buys-Ballot—that at the contact of two metals, the movement of heat itself causes potential differences. Thomson has given a theory of thermo-electric currents, which is complete, if we assume that the Peltier effect will show itself in one and the same metal at different temperatures. Tait has proved the well-known formula of Avenarius, and Edlund has proved the formula of Thomson—that the electromotive force of a thermo-element at the same difference of temperature is proportional to the absolute temperature. In the rational explanation of thermo-electric phenomena there are inconsistencies: (1) Thomson's careful experiments still leave it doubtful whether we really find Peltier's effect showing itself in one metal; (2) bismuth is positive to antimony, and yet the current flows in the heated junction from bismuth to antimony; (3) Peclet has proved that with increased temperature the contact difference of zinc and gold is unchanged.

(1.) The author describes how, using weak currents, he gets the result which Thomson obtained using strong currents. (2.) The author gives lists of metals placed according to experiments such as those of Volta and according to thermo-electric experiments. He finds by his own experiments the following contact differences with brass, given in percentages of a Daniell:—zinc 80, bismuth 16, antimony 12·5, silver 4, platinum I. 2·0, copper 1, platinum II. 4·0, gold 8. Platinum I. was chemically pure; platinum II. was the metal of commerce. This list, deduced from the author's careful experiments, he says, proves that the results of Volta, Seebeck and others are correct, in spite of the obvious carelessness of their methods. He goes on to show that,

for instance, in a bismuth-copper junction, the electromotive force produced in the bismuth itself tending to give a strong current from cold to hot parts of the metal, is actually greater than the contact electromotive force of the two metals; and hence, since there is only a small effect of this kind produced in copper, these metals ought to be differently placed in a contact list and in a thermo-electric list. This is also the reason why we find the neutral points in thermo-electricity. The author gives an experiment bearing out the above explanation. (3) He repeats Peclet's experiment, and gets a result which is in accordance with theory. He, therefore, concludes that potential differences shewn on heating are only modifications of those discovered by Volta; and as thermo-electric phenomena are explainable by the laws of thermo-dynamics he uses to explain Volta's contact differences, heat effects at the junctions. He deduces from this that not merely chains of metals, but chains of metals and liquids, should owe their electric currents to heat effects at the junctions; so that we must not explain them by changes in chemical effects. He discusses the case of zinc in sulphate of zinc, where there is an electromotive force of contact, and yet in which there is no chemical action. He describes an experiment showing that the positive electrode in a decomposition cell becomes warmer than the negative. The author describes arrangements by means of which zinc plates were maintained at different temperatures in sulphate of zinc, and copper plates in sulphate of copper, and he finds that the ratio of the electromotive forces of these cells is 1·26. He also finds that the thermal currents between metals and fluids depend, as regard their direction, only on the fluids, and not on the nature of the metals.

The author, after declaring that the question—Is the electromotive force of Daniell equal to the sum of—

$$Zn \mid Cu, \quad Zn \mid Zn\,So_4, \quad Cu \mid Cu\,So_4$$

has not hitherto been taken up, proceeds to describe his experiments proving that this is true. He finds—

$$Zn \mid Zn\,So_4 = 48 \text{ per cent. of a Daniell.}$$
$$Cu \mid Cu\,So_4 = 27{\cdot}4 \quad \text{,,} \quad \text{,,}$$

He then proceeds to consider the well-known statement that the direction of the current in a cell depends on the nature of the chemical effects producible at the junctions, and he shows by the example of a single cell formed of lead and copper in water that the statement is not always true. Thus, according to Favre—

$$Pb\,O - H_2\,O = 27{,}675 - 34{,}462 = - 6{,}787 \text{ heat units.}$$
$$Cu\,O - H_2\,O = 21{,}885 - 34{,}462 = - 12{,}577 \quad \text{,,}$$

and yet this cell gives a pretty strong current, which, in the fluid, has the direction from lead to copper.

The remainder of the paper is devoted to a theory of the voltameter, based on the following principle:—When a certain amount of polarization is in existence, not the maximum amount, it is as if one of the platinum plates had a number of buttons of zinc partially covering its surface, so that the current

from a voltaic cell when it enters the voltameter is in parallel circuit. In one path through the voltameter there is no opposing electromotive force, but by the other there is such an opposing electromotive force, and this latter becomes more and more important as time goes on. He deduces the result—

$$J = \frac{E - \frac{p}{n}}{R + r}$$

Where $p$ is maximum polarisation, E the electro-motive force of battery, $r$ resistance of voltameter, R all other resistance of the circuit, J the current, and $n$ a number which depends on the time decreasing from infinity to 1 with a rapidity determined by the current and the area of platinum plates. The author describes many experiments illustrating his theory—refers to Edlund's experiment showing that the heat effects produced in a voltameter are not merely those due to current resistance but also to polarisation—refers to the fact that Favre found the simple cell zinc, sulphate of zinc, platinum, to cool when it produced a current, and shows how other facts of the same kind are explainable on thermo-dynamic principles.

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## GUSTAV HOFFMANN—CHANGES PRODUCED IN THE STRENGTH OF IRON WIRES BY THE PASSAGE OF ELECTRIC CURRENTS.

(*Elektrotechnische Zeitschrift, H.* 5, *May, pp.* 155-162.)

In a preliminary history of the subject the author mentions the names of Dufour, Wertheim, Edlund, Streinitz, and Exner, and shows that the results were contradictory. He gives a Table of Dufour's results. His own results show that (1) the passage of a current through iron wire causes an increase of strength which depends on the diameter and on the constitution of the wire; (2) with a given current the increase of strength depends on the time during which it flows tending to reach a maximum; (3) with weak currents the increase of strength is proportional to the current. The following Table has been compiled from the author's results of experiments on eight iron wires, each one metre in length:—

| Diameter in Millimetres. | Average Breaking Weight, in Grammes, without Current. | Increase of Strength, in Grammes, due to the Passage of the Current for | | | Increase in Strength, in Grammes, due to Passage for equal times, of Currents of the following Strengths. | | |
|---|---|---|---|---|---|---|---|
| | | 3 hours. | 12 hours. | 24 hours. | 4 | 6 | 9 |
| 0·19 | 2,368 | 28 | 44 | 50 | 20 | 29 | 44 |
| 0·23 | 3,447 | 23 | 42 | 56 | 28 | 36 | 42 |
| 0·26 | 4,435 | 86 | 119 | 130 | 64 | 89 | 119 |
| 0·21 | 2,286 | 87 | 101 | 107 | 33 | 57 | 101 |
| 0·24 | 2,682 | 71 | 122 | 136 | 62 | 96 | 122 |
| 0·31 | 4,386 | 12 | 23 | 23 | 11 | 16 | 23 |
| 0·32 | 4,358 | 30 | 33 | 35 | 22 | 27 | 33 |
| 0·39 | 7,398 | 92 | 141 | 180 | [illegible] | 11[illegible] | |

In seeking for an explanation of these results we must distinguish between the effect of the heat produced by the current and the effect produced by the current itself. Possibly there may be an increase in cohesion due to heating, but certainly the mere passage of the current plays an important part in the phenomenon observed. The author shows that this is proved by the above numbers, and he refers to a probable connection between electric phenomena and the molecular motions in the iron wire. Villari was the first to show that an induction current takes place in an iron or steel rod which had been conveying a current when the rod is vibrated afterwards. Wiedemann as well as Thomson, Beetz, Herwig, and Chwolson obtained analogous phenomena. The author gives his idea of the molecular cause of these phenomena.

## W. C. RÖNTGEN—THE NEW RELATION BETWEEN LIGHT AND ELECTRICITY DISCOVERED BY DR. KERR.

(*Annalen der Physik und Chemie, No IV., B. X., H. 1, May 1st, 1880, pp. 77-92.*)

The author states that in 1873 he made experiments upon glass and Canada balsam, but found no results such as Dr Kerr had published on the production of double refraction by static induction. More lately he has repeated Dr. Kerr's experiments, using a lime light and Nicol's prisms of large aperture, and he has confirmed Dr. Kerr's results that the electricity exercises the maximum effects when the lines of force and the plane of polarisation of the lights make an angle of 45°, and that there is no effect whatever when they coincide or are at right angles. Various experiments were made with a strained glass compensator, and it was found that carbon bisulphide acted like glass extended along the lines of force, cod-liver oil like glass similarly compressed. With partially conducting liquids effects were obtained when an air-spark was interposed in one of the wires, and the electrical machine connected with a Leyden jar; a flash of light was then seen on the field corresponding with each spark, and showing that there is a momentary state of strain in conductors before spark passes. With a high vacuum across which no discharge could be made to pass, no optical defect could be observed, even when a very large electro-motive force was employed. The author's experiments agree with those of Dr. Kerr's in showing that different liquids under electric strain act on light like uniaxial crystals, having for their axis the direction of the lines of force, and that, like crystals, they vary from strong to weak and from positive to negative.

## F. H. VARLEY—IMPROVEMENTS IN THE PRODUCTION OF THE ELECTRIC LIGHT.

(*Zeitschrift Angewandte Elektricitätslehre, Vol. 2, No. 9, pp.* 203-210.)

The author describes how he has divided the Electric Light. In place of Carbon Points he uses continuous columns of finely powdered graphite or carbon, continually renewed. One method—the powder falls from a platinum or iridium funnel like the sand in a sand-glass, the electricity passing in the direction of the falling particles. The falling stream of carbon is in a closed vessel in a vacuum or a carbonic acid atmosphere. By mixing the carbon dust with borax gravel, &c., the resistance may be varied. In producing the current an arrangement, which seems somewhat like a Ruhmkorff's coil is employed—he also describes the use of a battery to work a magneto-electric machine to drive a Holtz machine and from this the supply of electricity is obtained.

# JOURNAL

OF THE

# SOCIETY OF TELEGRAPH ENGINEERS.

VOL. IX. 1880. No. 34.

The Ninety-first Ordinary General Meeting of the Society was held at the Society's Rooms, No. 4, Broad Sanctuary, Westminster, on Wednesday evening, November 10th, 1880, when the President, Mr. W. H. PREECE, received the members and introduced them to the Ronalds Library, which was thrown open on this occasion for the first time.

No formal proceedings inaugurated the opening of the Library, but the trust-deed was on view, in which the Trustees had certified to the fact that the Society of Telegraph Engineers had duly fulfilled the conditions of Sir Francis Ronalds' bequest. A large number of members and visitors attended the meeting, which resolved itself into a conversazione.

A number of rare and curious books relating to electricity, magnetism, navigation, &c., were exhibited, a list of which is subjoined. The new American Rapid Telegraph, of which a short account is added, was also shown in action.

## FROM THE RONALDS LIBRARY.

1. AURIFABER, ANDREAS. Succini historia, &c. 1551.
   [Contains references to the electrical properties of amber. Specimen of early binding. Scarce.]
2. PEREGRINUS, PETRUS. De Magnete, &c. 1558.
   [The earliest known book on magnetism. Very scarce.]
3. TAISNIER, J. De Natura Magnetis. 1562.
   [A piracy of Petrus Peregrinus. Rare.]

4. DIGGES, THOMAS. A prognostication of right good effect. 1592.
[Meteorological. Very curious.]

5. GILBERT, DR. De Magnete. 1600.
[One of the earliest and most important books on magnetism. Three editions, 1600, 1628, 1633.]

6. BARLOWE, WM. Magneticall advertisements. 1616.

7. RIDLEY, MARK. Magneticall bodies and motions. 1618.
[Early English book on magnetism.]

8. CABEUS, NICOLAS. Philosophia Magnetica. 1629.

9. GELLIBRAND, HY. Variation of the Magneticall needle. 1635.
[Very scarce.]

10. WARD, S. Magnetis reductorium. 1639.

11. KIRCHER, A. Magnes sive de Arte Magnetica. 1641.

12. ——— Physiologia Experimentalis. 1680.

13. OTTO DE GUERICKE. Experimenta Nova. 1672.
[Inventor of the air-pump.]

14. STURMIUS, J. C. Collegium experimentale sive curiosum. 1676.
[Curious work.]

15. MUSSCHENBRÖCK, P. VAN. Tentamina experimentorum naturalium. 1731.

16. DESAGULIERS. Dissertation concerning Electricity. 1742.
[Earliest English book on electricity.]

17. SCARELLA, J. B. De Magnete. 1759.

18. VOLTA, A. De vi attractiva. 1769.
[Volta's first work.]

19. BECCARIA, G. Elettricismo Artificiale. 1771.

20. MILNER, THOS. Experiments in Electricity. 1783.
[Containing the earliest description of the electrometer known as "Peltier's." Scarce.]

21. RONALDS, SIR F. Correspondence, &c., relating to the Electric Telegraph. MSS. 1816-1873.
[This interesting collection contains the letter from Sir J. Barrow, the Secretary of the Admiralty, saying that "Telegraphs of any kind are now wholly unnecessary," together with Ronalds' letters to the Government, &c.]

22. ——— Description of an Electrical Telegraph. 1823.
[One of the most interesting books connected with the history of telegraphy. The first English book on the electric telegraph.]

23. COLLECTION OF ENGRAVED PORTRAITS OF EARLY ELECTRICIANS.

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## EXHIBITED BY LATIMER CLARK, ESQ., M.I.C.E.

*Past President.*

24. VINCENTIUS BELLOVACENSIS. Speculum Naturale. 1473.
[A fine specimen of early printing; contains perhaps the earliest printed allusion to the polarity of the magnetised needle and its use by mariners.]

25. ALBERTUS MAGNUS. 1494.
[Good specimen of early printing, with allusions to the loadstone.]

26. PLINY, C. Natural History. Fo. 1497.
[Contains allusions to the loadstone, amber, the torpedo, and lightning.]

27. BLONDUS, M. A. De Ventis et Navigatione. 1546.
[An early tract on navigation—contains an engraving of a new mariner's compass called "Pixis vel buxolus" from (*Buxus*, box); from this the Italian word *bussola*, and French *boussole*, are derived.]

28. AGRICOLA, G. De re metallica. 1556.
[Curious metallurgical work.]

29. CARDANUS, H. De rerum varietate. 1557.
[Contains several early allusions to magnetism.]

30. PORTA, J. B. Magia Naturalis, &c. 1558.
[Contains at page 90 the earliest allusion to the imaginary sympathetic magnetic telegraph, formed by two similar mariner's compasses with letters round their margin, which was afterwards so frequently alluded to by the old writers, and among others by Strada, and translated by Addison in the *Spectator*. It is alluded to more fully in the later editions.]

31. PTOLEMAEUS. Geographia. 1562.
[Interesting geographical work. Ptolemy, who wrote in the first century, originated the story of the magnetic mountains in the Indian Ocean, which drew the iron nails out of the ships. The earlier editions were printed before the discovery of America, which is not shown on the maps.]

32. BIRINGUCCIO, C. La Pyrotechnie. 1572.
[Curious old work on metallurgy.]

33. NORMAN, ROBERT. The Newe Attractive. First edition 1582.
[The earliest work on magnetism, very scarce and much esteemed. It describes the declination of the magnetic needle.]

34. BARLOWE, W. The Navigator's Supply. 1597.
[A curious tract describing the mariner's compass.]

35. WRIGHT, E. Errors in Navigation. 1599.
[A scarce and early work, containing description of the mariner's compass and its use.]

36. DE SUNDE, J. H. Steganologia and Steganographia. 1600.
[One of the early writers on the imaginary magnetic telegraph, and the most curious. He describes in cabalistic fashion the preparation of the two compasses, the needles of which must be made from the same piece of steel and magnetised by the same magnet. Attention is called by the ringing of bells placed upon the dials and rung by bar magnets. The needles are also actuated by bar magnets, and the letters are formed by one, two, three, or four movements, as in the modern single needle telegraph. His real name was Daniel Schwenter.]

37. STRADA, F. Prolusiones. 1617.
[Contains the poem on the Imaginary Lovers' Telegraph, afterwards translated and published in the *Spectator*, Dec., 1711.]

38. BLUNDEVILLE. On Navigation. 1622.
[Contains, at p. 681, an early description of the manufacture of compass.]

39. Van Etten, H. Mathematical Recreations. 1633.
[Contains, at p. 104, the earliest English description and figure of the sympathetic telegraph. This work was really written by Father Leurechon in 1626, and was subsequently translated into various languages—many editions—all containing allusions to the sympathetic telegraph.]

40. Galilæi, G. De Systemate Mundi. 1635.
[The celebrated astronomer; refers, at p. 88, incredulously to the sympathetic telegraph.]

41. Browne, T. Pseudodoxia Epidemica. 1646.
[Contains, at p. 76, some interesting remarks on the sympathetic telegraph.]

42. Schott, G. Schola Steganographica. 1665.
[Contains, at pp. 258-260, description of De Sunde's sympathetic telegraph, with drawing.]

43. Glanvil, J. Scepsis Scientifica. 1665.
[Contains, at p. 149, a very interesting allusion to the imaginary magnetic telegraph.]

44. Frederici, J. B. Cryptographia. 1685.
[Contains, at p. 234, the earliest specimen of the Morse code.]

45. Wilkins, J. Mercury, or the secret and swift messenger. 1694.
[Contains many curious and interesting descriptions of secret writing, and of the imaginary telegraph.]

46. Zahn, J. Specula fisico mathematico historico. Folio. 1696.

47. Abercorn, Earl of. Attractive Virtue of Loadstones. 1729.
[A rare and interesting tract, giving tables for ascertaining the price of loadstones.]

48. Swedenborg, E. Principia rerum naturalium. 3 Vols. 1734.
[Contains a remarkable experimental treatise on magnetism. The author is well known for his singular theological views.]

49. Swammerdam, J. Biblia Naturali. 2 vols. folio. 1737-8.
[Describes, at a very early date, the galvanic effect of metals on the legs of the frog.]

50. Scots Magazine for Feb. 1753. 1753.
[Contains, at p. 73, a letter by C. M. (*i.e.*, Charles Marshall), in which a real electric telegraph is for the first time invented and described.]

51. Wesley, John. Electricity made plain and useful. 1778.
[The author was the founder of Wesleyanism.]

52. Marat. Recherches physiques sur l'électricité. 1782.
[The author is the same who figured so prominently in the early days of the first French Revolution, and who was assassinated by Charlotte Corday.]

53. Ohm, G. S. Die Galvanische Kette. 1827.
[Original publication of Ohm's law.]

54. Green, G. Mathematical analysis to the theories of electricity and magnetism. 1828.
[Contains, at p. 9, the earliest introduction of the word "potential," as applied to electricity. Extremely rare.]

55. Chronological Catalogue of Mr. Latimer Clark's Library. 1878.

56. Portraits of Electricians.

57. Autographs of Eminent Electricians.

# THE AMERICAN RAPID TELEGRAPH INSTRUMENT.

The American rapid, or "electro-mechanical," telegraph instrument has only recently been brought over to this country, with the view of its introduction on the Postal Telegraph system. The essential principle of the rapid is identical with that of the well-known Bain instrument. The apparatus consists of an ingeniously-arranged perforator (which punches slips after the fashion of the Wheatstone perforator), a transmitter, and a receiver. The transmitting slip is prepared by the operator playing upon a double row of keys, the depression of any one of which perforates at one blow holes in the slip representing the letter of the key depressed, the power being obtained by a treadle arrangement. The perforations are made in two rows, one on each side of the paper ribbon. In the transmitter two platinum brushes, in electrical connection with each other, make contact through the perforations of the sending slip with the metallic drum underneath. This drum is divided into two parts, one part being in connection with the copper pole of the battery, and the other with the zinc pole, the centre of which is permanently in connection with the earth. This arrangement is virtually that of an automatic double-current key, one brush sending positive, and the other sending negative currents to line. In the receiver the drum is not divided, nor has it any direct electrical connection. Two styles of iron (or steel), insulated from each other and kept a short distance apart, press upon the receiving slip. One of these styles is in connection with the line and the other with the earth. The current passes as it were across the slip, making a mark on one side for a negative current, and on the other side for a positive one. By this arrangement the signals are received either above or below the centre of the slip, and the spacing, which is necessary by the old method, is dispensed with. The paper slip on which signals are received is saturated with a solution consisting of proportionate parts of muriate of ammonia, nitrate of ammonia, red prussiate of potash, and water, and when the currents pass through the paper the solution is decomposed, a portion of the iron or steel stylet being dissolved, forming Prussian blue, which renders the signals visible.

The apparatus is capable of transmitting, under suitable conditions, at the rate of 1,500 words a minute, and while on exhibition before the Society a slip representing 1,100 words was received in 57 seconds. The above and below arrangement of signals on the slip seems confusing at first sight, but no doubt facility of reading is quickly gained by experience, as it is much assisted by the marks used to separate letters and words.

The manipulation of the perforator is not said to be faster than that of the Wheatstone perforator, but by making the perforations in the peculiar form for which it is arranged, it enables the signals to be sent so as to come out on the receiving slip in the manner described.

An automatic regulating brake is attached to both perforator, transmitter, and receiver, and by its action tolerable uniformity of speed in the revolving mechanism is secured. A condenser also is attached to the transmitter, which sends currents to line while the transmitting brush is passing over the short spaces between perforations, and ensures a continuous mark being made to represent a dash when required, instead of a series of dots representing a dash. The apparatus is the joint invention of Messrs. Foote, Randall, and Anderson, of the United States, where it is now used by the American Rapid Telegraph Company between Boston and New York, a distance of 250 miles.

The Ninety-second Ordinary General Meeting of the Society was held on Wednesday evening, November 24th, 1880, at the Institution of Civil Engineers, 25, Great George Street, Westminster—Mr. W. H. PREECE, President, in the Chair.

The minutes of the previous meeting were read and confirmed, and the names of new candidates announced, also that the following Associates had been transferred to the class of Members:—

James D. Doyle.
E. J. Paterson.
E. March Webb.

The PRESIDENT: Gentlemen,—It is an interesting fact that I have to announce to you, viz., that at our last meeting the Trustees of the Ronalds Library, who were present, unanimously decided that this Society had completely and satisfactorily complied with all the requirements of the deed of gift by which such a splendid library was given to us. The subject of the opening of the Library to the members, and also, under proper regulations, to the public, is receiving the consideration of the Council, and I hope that by the General Meeting, which will take place on December 22nd, a proposal will be submitted to the members which will meet with the general approval of the whole Society.

It is now my great pleasure to introduce to you Mr. J. W. Swan, from Newcastle-on-Tyne, who has, in his own opinion, solved that very difficult question of the sub-division of the electric light, and will exhibit lamps constructed on his new principle.

## THE SUB-DIVISION OF THE ELECTRIC LIGHT.

By J. W. SWAN.

Electric lighting by means of the arc, and by the incandescence of thin abutting carbon rods, has recently been discussed in an exhaustive manner at two of your meetings. The one other mode of electric lighting besides the two I have mentioned, namely, by the white heat of a continuous conductor, was barely glanced at in these discussions. This method of electric lighting is, however, theoretically so good, that, supposing the practical difficulties

which surround it can be surmounted, it instantly assumes a position of importance far above that of the methods upon which you have so recently bestowed exclusive attention.

At the invitation of the President, I have undertaken to place before you, very briefly, some of my ideas on this neglected branch of the subject, and to exhibit to you some of the practical results at which I have arrived.

Everybody in the least acquainted with the subject knows, that if we exclude from consideration this last-named method, lighting by means of electricity has an extremely limited range of application.

Electric light, as produced by the arc, in foci of great luminosity, is unquestionably an economical light when concentration of light is advantageous, but even then, the mechanism necessary to counteract its tendency to vary in power, the noise which is apt to accompany it, the trouble and cost of the replacement of the carbons, are inherent defects of such magnitude, as must always greatly diminish, and frequently entirely neutralise, the advantages of its economy and extreme brilliance.

Where concentrated and powerful light is inadmissible, and an equable distribution of light by means of *small* light centres is required, there the electric arc is totally inapplicable. It is impossible by that means to produce economically such a small unit of light, as in nine cases out of ten, or even in ninety-nine cases out of a hundred, is required for practical use. The moment you attempt to produce a small arc light, you sacrifice its chief redeeming quality, namely, its economy.

In those cases where the arc light on a large scale, for lighting large spaces, is not inapplicable, much of the economy of the light, considered merely with reference to its aggregate amount, is lost by the diminished power of the light at the extreme margin of the area of illumination.

I agree with the President in thinking that it is not so much the power of the light at its focus that has in most cases to be considered, as the amount of light at the least illuminated points of the space illuminated. Just as we say "the strength of a chain is its weakest link," so we may as truly say, that where the

general illumination of a certain area is in question, the value of a particular mode of lighting must be measured by the amount of light at the points of least illumination. Whether we agree with you, Mr. President, or not, as to the ratio of loss in sub-dividing the arc light, I am certain that the opinion I have already expressed will not seriously be contested by any one here, namely, that when you come below a rather large unit of light, you cannot sub-divide the arc without great loss. Looking at the matter from this point of view, it appears to me that the only direction in which we can move with any chance of substantial progress towards the making electric illumination generally applicable, is the direction I have taken, namely, that of abandoning the arc altogether, and going to incandescence pure and simple. On the principle of incandescence, and on that principle alone, you can produce with economy such a small unit of light as is required for domestic use and general purposes. On the principle of incandescence alone, can you economically divide electric light, and on that principle you *can* divide it indefinitely. By means of incandescence it is possible to produce a single light, say, of 100 candle power; and with precisely the same expenditure of energy, the same aggregate amount of light may be produced in say ten separate places.

Being in your meeting-room at Broad Sanctuary with a few minutes on my hands, and with no one present to say me nay, I took the liberty of taking a volume from one of the shelves of your most excellent library; it was the first volume of Faraday's *Experimental Researches in Electricity*. I first turned to the index and then to page 250, and there I read this: "The same quantity of electricity which, passed in a given time, can heat one inch of platinum wire, of a certain diameter, red hot, can also heat a hundred, a thousand, or any length of the same wire to the same degree, provided the cooling circumstances are the same for every part in all cases."

Speaking as I do, to Electrical Engineers, who have the bearings of Ohm's law at their finger ends, it is not necessary to elaborate this point. You, of course, all recognise the fact that a specific degree of incandescence produced in a certain wire by the trans-

mission of an electric current is indicative of a specific-current flow, and whether a short piece of the wire is heated to the supposed degree, or a long piece, the same current flow will be required in both cases; and, other circumstances being alike, whether one inch is heated, or one hundred inches, the *only condition* necessary to be observed in order to the maintenance of a constant current, and consequent constancy of incandescence in the wire, is variation of the electro-motive force exactly as the length of the wire varies.

I may therefore say, in strict accordance with theory, that the indefinite division of electric light produced on the principle of the incandescence of a resisting and refractory conductor is unattended with loss. The only question is the all-important one of the practical realisation of that which theory says is possible.

It is undeniable that great practical difficulties attend the economical application of the principle of incandescence. Platinum, iridio-platinum, and carbon are the chief substances available for producing light on this principle with which attempts have been made. The difficulty with platinum and with iridio-platinum is, that they fuse or break before they attain a temperature of economical incandescence; and when it is remembered that after a certain degree of incandescence is attained, increase of current produces much more than a corresponding increase of light, it will be evident that it is vital to the economy of this mode of lighting to be able to heat the incandescent body to a very high degree. Mr. Edison has stated that by frequently heating and cooling a platinum wire in vacuo, it sustains a very much higher temperature without fusion (perhaps it would be more strictly accurate to say, without *rupture*), than when not so treated. Notwithstanding this improvement in the adaptation of platinum to incadescence-lamps, and notwithstanding also some very ingenious contrivances to guard against fusion of the wire, I think, perhaps, one is warranted in the assumption, that the abandonment of this form of lamp, and the taking up with carbon instead of platinum, is an acknowledgment of its non-success.

Carbon has been more attractive to experimenters in this field

of research than platinum, and a much more tangible result has been attained by its mediation.

Since 1845, when King proposed to produce electric light by means of a continuous carbon conductor intensely heated by the passage of a current of electricity, there have been numerous attempts to realise a useful form of electric light by means of white-hot carbon.

It is needless to say that, in order to make the carbon in an incandescent lamp give forth a useful amount of light, it must be made very hot, so hot that it would immediately burn if there were air present. There are lamps, generally called incandescence-lamps, in which no precaution is taken to prevent combustion, such are the Werdermann, Reynier, and André lamps. These lamps ought not to be classed as incandescence-lamps in the sense in which alone I use that term, viz., to describe lamps in which there is white heat without combustion. No extensive economical sub-division of the light by lamps in which there is combustion is to be hoped for. The true incandescence-lamps prevent the combustion of the carbon in one of two ways, either by the entire exhaustion of the air from the chamber in which the heated carbon is placed, or by the filling of the chamber with an inert gas, such as nitrogen. Both these expedients were tried by the early experimenters, and both have still their advocates.

Many of the older attempts to utilise carbon as the medium of incandescence, for a time, appeared to be successful; but eventually they disappointed the hopes they had raised at the outset. They failed from three causes, any one of which was sufficient to cause failure. First, the carbons employed were so thick as to require a large current to produce the required temperature in them, and consequently the light was not economical; second, the carbons were not durable; and, third, the lamp-glass speedily became obscured. It is long since I attempted to grapple with the first of these difficulties.

As a matter of history, I will briefly describe an experiment which I tried about 20 years ago. I had a number of pieces of paper and card of various forms and sizes buried in charcoal in a crucible. This crucible I sent to be heated white-hot in one of

the pottery kilns belonging to Mr. Wallace of Forth Banks, Newcastle. From the pieces of carbonized card which I thus obtained I selected a long spiral; the ends of this I clipped between small blocks of carbon carried by uprights and connected with conducting wires. A small glass shade was cemented over this mounted carbon spiral, and the air was exhausted by means of a very good air-pump lent to me for the purpose of this experiment by the Rev. Robert Green of Longhorsley. A good vacuum (according to the ideas that then prevailed) having been produced, I applied the wires of my battery (consisting of 10 cells of Callan's modification of Grove's battery), with great expectation of a brilliant result: instead of this there was the most absolute negative presented to me; not a vestige of heat or light appeared in my long ringlet of carbonized paper. It was evident, and I immediately recognised the fact, that the electric current of the strength I was using would not go in sufficient quantity through so long a piece of carbon as I had taken. I therefore repeated the experiment with shorter carbon and a greater number of cells, and I obtained, under these altered circumstances, an extremely interesting result.

My carbon was in the form of an arch about one inch in height and width, and the strip forming the arch a quarter of an inch broad. The ends of the arch were held in small clamps, with square blocks of carbon.

The air pump having been worked, I had the pleasure of seeing that when contact with the battery of 40 or 50 cells was completed, my carbonized paper arch became red-hot, and it was evident that nothing more was wanted than a still stronger current, to make it give out a brilliant light; but I had used up all the battery power at my disposal, and having reached this limit, I contented myself with watching the behaviour of the arch, the engrossing question being—how long will it endure? I noticed that the inner part of the arch was hotter than the outer part, and that, perhaps in consequence of this, the arch became bent on one side. This bending gradually increased, until at last the arch had so far curled down that the top was on a level with the clamps, and on coming in contact with the sole of the lamp it broke in two, and the experiment collapsed.

That, I confidently believe, was the very first instance in which carbonized paper was ever used in the construction of an incandescent carbon lamp. I am now speaking of twenty years ago, and at that time the Voltaic battery was the cheapest source of electricity known, and the means of producing high vacua were very much less perfect than they are now.

I laid my electric experiments aside until about three years ago, when two things concurred to lead me to pursue the subject afresh. The discovery of the dynamo-electric machine had entirely altered the position of the question of electric lighting, shifting it out of the region of things scientifically interesting into that of things practically useful. The Sprengel air-pump, too, had been invented, and with its invention we had been provided with a means of producing much higher vacua than could be produced by the old form of air-pump. Mr. Crookes' radiometer experiments had shown us what a really high vacuum was, and how to produce it. Mr. Stearn, of Birkenhead, an ardent scientific amateur, was so attracted by the extraordinary results Mr. Crookes had obtained by means of high vacua, as to go with great enthusiasm into the same line of experiment, and he soon acquired such a knowledge of the Sprengel pump, and such expertness in its manipulation, as perhaps was only equalled by Mr. Crookes himself. I had the good fortune to make Mr. Stearn's acquaintance, and that was the other one of the determining causes of my second attempt to solve the problem of electric lighting by the incandescence of carbon, for it is to the invention of the Sprengel pump and to the many admirable lessons in the use of it which Mr. Crookes has given us that we are indebted for the means we now possess of attaining to the condition essential to the successful employment, in electric lamps, of thin strips or filaments of carbon.

But if the employment of carbonized paper got rid of the difficulty as to the waste of current due to the thickness of the carbon, there still remained the other two difficulties, viz., the tendency of the carbon to disintegrate and break and the obscuration of the globe. How formidable those difficulties are may be judged of by the following extract from Fontaine's well-known treatise on electric lighting, in which he gives us the results of his experiments in the production of light by incandescence.

At page 180 this passage occurs: "Attentive examination of incandescent carbons through a strongly-coloured glass has shown that they are not uniformly brilliant; they present obscure spots, indicative of non-homogenity and the position of cracks, which rapidly disintegrate the carbon. The vacuum never being perfect in the receivers the first carbon is in greater part consumed. It would appear that consequently upon the little oxygen contained in the lamp being transformed into carbonic acid and carbonic oxide, the carbon should be preserved indefinitely; but there is then produced a kind of evaporation which continues to slowly destroy the incandescent rods. This evaporation is, besides, clearly proved by a pulverulent deposit of sublimed carbon that we have found on the interior surface of the bells on the several interior parts, rods, contacts, hammers, &c."

It never seemed to me conclusively proved that M. Fontaine's theory of the breaking of the carbons and the obscuration of the globes was correct, and I did not accept it. I remembered, for instance, how impossible Mr. Crookes had found it to produce a really high vacuum in vessels with luted joints, and I knew how commonly luted screw joints had been used in the fittings of incandescent lamps. I know, too, that the common air-pump had generally been employed as the means of exhausting the air. Then, too, the mode of attachment of the carbon to the conductors conveying the current to it had been such as must inevitably lead to excessive local heating, and even to the occurrence of disruptive discharge about the points of attachment.

Watching the course of events in relation to electric lighting experiments, and observing the very imperfect conditions under which generally those experiments were conducted—and that full advantage had not been taken of the new powers which the progress of scientific research had placed within our reach—I three years ago returned to the quest with the distinct purpose of finding whether or not carbon made from carbonized card, and carbonized paper, were durable in a really high vacuum, such as Mr. Crookes had employed in his experiments on the radiometer. This time I had, as I have mentioned, the great advantage of the assistance of Mr. Stearn, of Birkenhead, who in addition to being

exceedingly expert in the manipulation of the Sprengel air-pump, is an admirable experimentalist generally. Since then experiments without number have been made by us with the view of surmounting the various practical difficulties of which I have spoken, chiefly the breaking of the carbons when made highly incandescent, and the blackening of the globes. Tolerably good results were obtained with carbonized paper and carbonized cardboard, but latterly I have found a material (a form of carbon) which is very much better suited to the purpose. With this new material the lamps which I am about to exhibit to-night are fitted.

Here is one of my carbons. It is, you will observe, an extremely thin carbon wire about ·01 of an inch in diameter, and weighs about the ·02 of a grain to the inch. It is both hard and elastic, so much so, as more to resemble a fine steel wire than carbon. Its hardness and non-combustibility increase with lengthened use. After being used some time it becomes so difficult of combustion as to bear heating in a blow-pipe flame for a considerable time before it burns away.

I am able to shape this carbon wire into circles or spirals or almost any form, and to thicken the ends so as to make a good and extensive contact with the holding sockets. A current of less than a Weber going through this carbon wire raises it to a state of brilliant incandescence, and if the current much exceeds that amount then the light becomes splendid. I have not yet finally ascertained what is the greatest amount of light that, with sufficient regard to durability, can be obtained by means of one of those spirals of not more than $\frac{1}{10}$th of an inch total superficial area. I have taken a few photometric measurements, 60 candle-power from one lamp being the greatest amount of light actually measured; that degree of luminosity was produced with about one Weber of current, and 100 volts. E.M.F. I have seen at least double this light from each lamp. At Sir William Armstrong's, at Cragside, there were sixteen lamps hanging in the picture gallery, the current from a large Siemens' machine was divided between these and a resistance coil. For a minute or two the resistance coil was cut out by short circuiting, then the short circuit contact was broken and the resistance coil left out, so that

the full current from the generator, the potential raised to a very high degree by the short circuiting, rushed in a torrent through the sixteen lamps; there then occurred the most glorious outburst of light I ever beheld. It was truly splendid! It lasted I should say full five minutes, and then subsided to normal brilliance.

The economy of this method of producing electric light by the incandescence of carbon in vacuo is limited by the capacity of the slender conductor to endure the enormously high temperature corresponding to an extreme degree of incandescence. It is certain that the carbons are more durable when heated to a moderate degree than when their temperature is pushed to an extremely high point. It is, therefore, still an open question what is the most light that can be practically obtained by means of such lamps. We do however know this, that a well made lamp of this size will endure for several months if it be not pressed to give more than the light of 30 standard candles. It is quite likely when the manufacture is improved, as it will be by practice, that twice this amount of light may be exacted without too much limiting its durability.

The vacuum in the lamps is very high—in fact it is carried to that point at which it is practically non-conducting.

The lamps which are hung about the Theatre are arranged in parallel arc. I have elsewhere expressed the opinion, that with the lamps so arranged the current cannot be economically supplied at a great distance from the generator: to accomplish that end and render it feasible for current supply to be sent from a centre over a wide area, as gas through mains, it will, I think, be necessary to use a very high electro-motive force and arrange the lamps in series. The difficulties which are incident to that method are, I hope, not insuperable; extinction of a line of lamps from the failure of one of the lamps of the series, could be guarded against by a very simple form of automatic by-way, and the potential of the current could be made to rise or fall correspondingly with a variation of the resistance, and so as to maintain a constant current however few or however many lamps were lighted.

The resistance of the lamps obviously depends on the length and thickness of the carbon wire. Those I have spoken of as

emitting, when moderately heated, a light equal to 30 standard candles, have a resistance of about 100 Ohms when cold. I have made them of twice that resistance, but even then it would be impossible to feed at a distance a large group of lamps arranged in parallel arc with a small conducting wire.

I have said nothing about cost, beyond giving you one or two facts, which will afford you the basis of an approximately correct computation, and from which it will appear that while, by the incandescence of thin conductors of carbon, a certain current does not give so large a return in light as when employed to produce the electric arc between carbon points on a large scale, yet the amount of light developed by a given expenditure of power is amply sufficient to warrant the statement that it is an economical source of light.

I will now turn the current on to the lamps, and, if the arrangements which have kindly been made by Mr. Ward and Mr. Fleetwood are what I expect, we shall see how they behave in the presence of so critical an audience.

The PRESIDENT: It is not often, gentlemen, that we hear such a round of applause in this room as has greeted the completion of this admirable and excellent paper. But that applause was richly and deservedly merited, and no formal vote of thanks to Mr. Swan is needed; but to follow out our ordinary rule, I will ask one or two of those gentlemen whose names are associated with Electric Lighting to carry out our course of proposing a vote of thanks before the close of this meeting. But, first, I should like some of the members present to elicit further information from Mr. Swan by questions, or to add further interest to the paper by any comments they may wish to make upon it. There are present among us Gas Engineers, who may perhaps feel a little uncomfortable; there are also present Electrical Engineers, who are following the progress of Electric Lighting as a profession, or for the love of their profession, and I will ask each and all of such gentlemen to add interest to the paper, either by relating their original experience, or by criticising, if they can, the very admirable lecture we have just listened to. There is Mr. Crompton, for instance, whose name

has been mentioned already by Mr. Swan, and whom we all know, and who has recently been the successful competitor in an Electric Light Exhibition, with his arc lamp.

Mr. CROMPTON: When Mr. Swan first showed me this beautiful light, I felt for a moment as if I were a gas shareholder, and that my means of existence and vocation as a manufacturer of electric arc machinery was most seriously threatened; on second thoughts I saw that there was room for us all.

I have been to Newcastle and seen the light in its home, measured the currents used, and satisfied myself of the easy adaptability of the light for domestic use.

As regards the economy, I think it will be found that the light shown to-night, although not so economical as the large arc lights such as I have been using, will be more economical than the smaller arc lights used by those who subdivide the current to any extent. In comparing the economy with that of gas, I think Mr. Swan showed conclusively at Newcastle that a certain quantity of gas used in a Crossley gas engine to drive a Gramme dynamo-electric machine and 20 of Mr. Swan's small lights, gave a better illuminating effect than the same quantity of gas used through ordinary burners, in the ratio of 2 to 1. Perhaps, if the best Argand burners are used, the ratio would not be more than 1·5 to 1. But in making calculations of the relative economy of this new light, we must not lose sight of the fact that a very slight difference in the direction of reducing the resistance of these delicate filaments of carbon, by shortening them, makes an enormous alteration in the amount of light obtainable from a given current power. If I mistake not, Mr. Swan showed to me that by reducing the length of the filament one-half, thereby halving its resistance, a five-fold light was obtained. The question at once suggests itself, How far will the durability of the lamp be effected by this shortening of the filament? as of course the action over the smaller surface of carbon is enormously intensified. I think the problem Mr. Swan has now to work out is, the exact degree of current action to which he can submit the carbon and yet get a fair life out of it. There is no doubt that the public, when crying out for an electric light suitable for domestic purposes, do not look for a light somewhat more dingy

than the gaslight they now possess, and they will not be satisfied with units of ten candles such as the row of lamps on the small frame in the centre of the room. Mr. Swan will have to be prepared to improve his manufacture until he gets a lamp that will last for at least two months, standing such a current as is now being passed through the lamps on the large frame. With the above duration of two months only, I think such a lamp will easily beat gas in economy.

As an arc lighter, I do not accept Mr. Swan's sweeping statement that the uses for the arc light are very few: in my opinion they are very considerable, and increase daily. People are beginning to be no longer satisfied with having their streets, squares, and enormous railway stations dimly lighted throughout a stratum twelve feet high from the ground: they want the whole place illuminated from floor to ceiling, from pavement to the top of the houses. This can be, and is done by the arc light well and economically, and I do not think that Mr. Swan will be able to touch us here with his small incandescent lamps.

Dr. J. HOPKINSON stated that he did not think small arc lights were necessarily wanting in economy. The probable reason of the low economy actually obtained with small arc lamps was that they had generally been produced by an alternating current, and there were theoretical reasons why an alternating current should be less economical with an arc lamp than a continuous current. These reasons did not apply with the same force to an incandescent lamp.

Mr. ALEXANDER SIEMENS: I agree generally with Mr. Crompton that the arc light is not superseded by Mr. Swan's lamp; but Mr. Swan's remarks as to the applicability of the arc light ought to be judged in a different way than as put by Mr. Swan. If it is desired to illuminate a large space imperfectly, it may be done by a large number of small lamps, but if it is to be lighted brilliantly, in my opinion nothing better can at present be found than the arc light. Any one visiting the British Museum will find the reading-room of that institution illuminated about as perfectly as possible by arc lights. Small lights have been tried in the same room, but were found wanting. In point of economy I do not think that small lights, are as cheap as arc lights, but for small rooms and

where a division of the light is desirable, small lights only are applicable. As to the competition of the Electric Light *versus* Gas, I have very little to add, but think it is quite right on Mr. Swan's part not to imitate some American friends of ours and say that his lights would supersede gas at once. On the contrary, Mr. Swan throws out hope for the gas shareholders, and I believe myself that competition will increase the uses to which gas may be put, such as heating and cooking, which extension of the gas field will make room, perhaps slowly, but surely, for small electric lights for rooms and domestic illumination, but will not destroy the utility of the arc light for large areas and buildings.

Mr. BERLY had enjoyed listening to the paper, so far as his knowledge of the English language would permit him to do so, while it was being read. He would have some observations to make on the subject, but preferred to postpone them until he had carefully read the paper, and properly digested it. It was his opinion that the future would not belong entirely to small lamps, for if it were not desirable to concentrate light, the present system of gas lighting gave every facility for infinite subdivision; yet in many cases concentration by sun-burners and such like alone was aimed at.

Professor TYNDALL, F.R.S. : I have merely to congratulate Mr. Swan on the success of his long-continued course of experiments. They promise to be very important. The question remaining to be decided is one which he himself has touched upon, and that is the durability and freedom from accident, whether through shocks or through changes in the supply of electricity of these extraordinary filaments of carbon, regarding which my curiosity is something similar to that of Dr. Hopkinson. I believe that small and handy lamps such as Mr. Swan describes may possibly be turned to account in our coal mines. It would be easy to enclose such small lamps in water, so that they might thereby be protected from any possible connection with the explosive gases of the mine. We are therefore in a field which I trust may be explored with the view of rendering less numerous those shocking calamities which distress us from time to time. I will only repeat the wish that Mr. Swan may go forward to still further successes, and that

when they are duly tested he will find his filaments of carbon durable, and not liable to be easily deranged. This is the point of chief importance as regards the practical aspect of the question. Continued experiments will determine this point, and should it be settled satisfactorily, it may be said that Mr. Swan's success has been completed.

Mr. J. N. SHOOLBRED: Like previous speakers, I find it difficult, without any precise knowledge as to the nature and mode of manufacture of Mr. Swan's carbon lamps, to dwell upon the merits of the system, unless it is to admire the soft and pleasing effect produced generally, and to await a fuller acquaintance with the system before attempting to pronounce any definite opinion as to its merit relatively to that of other electric lights.

Upon one or two of Mr. Swan's remarks on the arc lights, made in the earlier part of his paper, I would, however, make a few observations. First, as to the absence of accurate information with the respect to the intensity of the arc light afforded by those systems which provide a number from a single electric machine.

It may be interesting to mention that, at the St. Lazare Station of the Western of France Railway, in Paris, a number of very accurate photometric measurements were carried out in 1878, by the officials of that railway, upon a series of Lontin lights; 18 of which, supplied from two sets of alternate-current machines of that system, were then in use at that station. Each light was found to have, on an average, an intensity of about 570 candles, measured on the horizontal.

I entirely coincide with Mr. Swan in deploring that the trials of the several systems of electric lights, at the recent exhibition of apparatus for lighting, &c., held at the Burnbank Drill Hall, Glasgow, were not carried out in their entirety, especially as regards the "Brush" system. For by this system are produced a number of moderate sized arc lights from one continuous-current machine; a counterpart, to a certain degree, of what is effected by the "Lontin" system with an alternating-current machine. An accurate knowledge of the intensity of each of the Brush lights, and of its proportionate expenditure of mechanical energy, would therefore, in the *interests of science*, have been of much value.

The proportion of candle-power per horse-power with the larger arc lights, one on a circuit, such as on the Gramme and on the Siemens systems, arrived at by accurate photometric and other measurements by several independent observers, may be taken as ranging from 1,000 to 1,200 candles, measured on the horizontal.

It may also be of interest to endeavour to form a rough approximation of the proportion of power taken by the lamps of Mr. Swan, now in this room. I am informed that they are supplied with the electric current from a "B" size Gramme machine. If so, it would probably be absorbing from 4 to 5 horse-power. There are before us twelve glass globes lighting the room, each containing three of Mr. Swan's incandescent carbon lamps, of probably 20 candle-power each. Hence we shall have $12 \times 3 \times 20 = 720$ candles' intensity altogether, which, being divided by 4 horse-power, gives 180 candles' intensity as the result of each horse-power expended; a result considerably less economical than with any of the arc lights.

Indeed, Mr. Swan's system, while extending electric lighting to the domain of domestic use, does not appear to interfere with the sphere of the ordinary carbon lights, more especially of the arc lights, the economy of which still remains undisturbed.

The success of Mr. Swan's system of electric lighting depends entirely, as has been said by Professor Tyndall and by other speakers, upon the durability of the lamp, so as to remain fit for use without being renewed for at least several months.

Professor AYRTON: Without troubling ourselves about the form of dynamo-electric machine or of steam engine which Mr. Swan is employing, the information he has himself given us will enable a calculation at once to be made of the relative economy of his incandescent lamps and a good arc lamp. One weber of current passing through an incandescent lamp, with an electro-motive force of 100 volts maintained on the two sides of it, produced, Mr. Swan tells us, 60 candles illumination. But this amount of electric energy we know is equivalent to 0·134 horse-power absorbed in the incandescent carbon filament. But one horse-power will with the arc produce a light of 1,200 candles, including the waste of energy

in the dynamo machine itself and in the leading wires, both of which have been omitted, and must be added to the consumption of energy in the previous calculation. If, then, the incandescent lamp be, as Mr. Swan tells us it is, absolutely cheaper than gas, how immensely cheaper must be the arc electric lamp.

It occurs to me that there is one reason which has been quite overlooked why the small trace of air left in the earlier forms of incandescent lamp always produced an ultimate rupture of the carbon filament, which is this: it was supposed that the little air would combine with the carbon and form carbonic acid gas, after which no further burning away of the carbon would take place. Such reasoning would be right enough if the carbon filament were at a lower temperature, but at the very high temperature which the centre of the carbon filament is at, dissociation of the carbonic acid gas takes place, consequently we have carbonic acid gas being constantly formed by the residual air combining with the carbon where it is below the temperature of dissociation, and the splitting again at the hottest portions of the carbon of the gas into carbon and free oxygen, ready to combine again with the colder portion of the carbon.

Experience alone can show whether in the Sprengel vacuum, such as Mr. Swan employs, there is still sufficient air left to produce this effect; but certain I am that, if any practical amount of air be left in the vacuum, the carbon will be burnt away.

Mr. MOULTON: There is a certain class of individuals who have a right to be heard upon this question of electric lighting. I mean the householders who desire a good light for domestic use. As one of that body, who after all are objects of some interest to those who are devoting themselves to the subject of electric lighting, I have looked eagerly for some form of electric light which should satisfy the simple but definite conditions to which a house-light must be subjected, but hitherto without success. To-night, however, this want has been satisfied. I do not think that any one can see the lamps around us without feeling that such lamps quite satisfy the wants of those who would be glad to see electric lighting in ordinary-sized rooms. Having then the suitable article, the question arises, Is it brought within our means? By that question

I do not mean what is the actual cost of the electric current nece sary to produce it. The question how many webers you can g through an ohm for a farthing is not the most important questio from our point of view. As I understand it, three of these lamp are kept going by one horse-power, and to persons who really lik a pleasant steady light, one-third of a penny per hour (which woul be the maximum cost on this hypothesis) cannot be considered ver extravagant. At all events, on *fête* days one might indulge onesel to an hour's electric lighting on such terms. Thus having brough the electric light within the price of luxuries, let us consider wha other expense a lamp like this is to a domestic consumer. In suc matters the main cost is due to the waste of time and trouble in th management of the lamps, and the expense of obtaining a fres lamp in case of breakage, and in finding or training servants who are sufficiently skilled to learn the details of the system, so that one can rely on the light with as great certainty as we now rely on gas. I do not want to be taught how Mr. Swan makes his lamps or filaments: I am only too glad that some one will make them for us. But I do want to know how Mr. Swan proposes that a lamp like this shall be made to work successfully when there are none of those skilled gentlemen present whom he mentioned as having given their valuable assistance to him on this occasion. Will he kindly tell us whether there is any difficult adjustment required in maintaining the connections of the lamp, or how these connections should be made? If he will just give a very brief sketch of the operations necessary to be performed in the morning, so as to fit up the lights ready for evening illumination, he will confer a very great boon on those who stand in a similar position to myself.

Mr. C. F. VARLEY: Like many of the speakers this evening, I would very much like to have some information from Mr. Swan which I am afraid he will not feel it his duty to give. I would like to ask him whether he has ascertained, and what is the temperature of the incandescent carbon threads when they are at their present, or what may be called their practical brilliancy? Also whether the carbon varies very much in its resistance at that temperature from what it was at the temperature of the atmosphere, which he said was about 150 ohms?

As to how he prepares the carbon, as to how he attaches the carbon, and how he gets his final "vacuum," of course we are all burning to know, but I presume that Mr. Swan will answer that he cannot at present tell us without betraying his secrets too much. Professor Ayrton has drawn attention to one subject which I think has been too much overlooked, and which is the cause of the carbons failing. If there be any gas capable of dissolving carbon present in the so-called vacuum surrounding the carbon thread, the high temperature will cause them to combine, and the difference of potential of different parts of the thread will set up electrolytic action. If it be oxygen that is present, carbonic acid and carbonic oxide will be first formed: this will be decomposed into oxygen on one side, and carbon on the other. The oxygen will then combine with a fresh portion of carbon, forming carbonic oxide, thus eating away the carbon thread at the more positive end. Every simple known gas combines with carbon. If it be hydrogen that is present, olefiant gas or marsh gas will be formed; if it be oxygen, there will be carbonic oxide or carbonic acid formed; if it be nitrogen, cyanogen will be produced; if chlorine, chloride of carbon; and I do not think there is any gas or vapour that we can produce (unless it be the vapour of one of the metals) that will not corrode the carbon when it is incandescent. And therefore, in order to exclude the uttermost trace of any gas that might combine with the carbon, I would suggest to Mr. Swan the use of an attenuated vapour of some non-decomposable metal.

A very great deal of difficulty is experienced by outsiders in understanding the value of a certain light. It is customary to compare an electric light with so many standard candles. This process of comparing any great illuminant with a candle is a very fallacious one. For example: if two or more common fishtail burners be allowed to burn in a room behind an observer's back, and he is looking at a sheet of white paper illuminated by those burners, when the two flames are brought into contact the paper suddenly seems to receive a great accession of light. A critical examination of that light, however, will show that the colour has been reduced or degraded from a whitish light into a much redder light. And now comes the question as to the value of light of

different colours. Any person paying attention to the subject will find that a red light is much more stimulating to the eye than what is termed a white light, and therefore, when an electric light is used to illuminate, a greater amount of light is actually required to stimulate the eyes. This is one reason why the electric light is not likely to be so economical as gas light; we want more of it.

The great features of Mr. Swan's light is that of being steady and capable of distribution into a great number of small lights. The illuminating power of a lamp decreases with the square of the distance, therefore the lamp that will give sufficient illumination at one hundred yards will give ten times ten, or one hundred times, more light than is required at a distance of ten yards. Therefore, the greater number of lights, the better and the more uniform is the illumination. A large number of small lights will therefore be more economical than one or two enormous lights fixed a great distance apart. For lighthouses one small light is all that is required, and that light must be as bright as possible; but for ordinary illumination a large number of small lights is evidently the system that must be adopted.

Mr. SWAN, replying, said: Mr. President and gentlemen,—I beg to thank Mr. Varley for the valuable suggestion he has thrown out with reference to the means of improving the vacuum of the lamp and rendering it inert towards the carbon. No doubt Professor Ayrton and Mr. Varley are theoretically correct in their remarks as to what goes on in the vacuum when the carbon is in a state of incandescence. But the practical fact is, the vacuum we employ is of such a character that the carbons do endure for a very considerable time. We have had some lamps burning (with an interval of three weeks, during which they were kept cold) since August 8th, *i.e.*, during the evenings.

I will just make a general response to the many requests that have been made that I should state precisely in what manner I make the carbon filaments, by saying that I have patented the method, or, rather, application for a patent has been made, and is on the point of issue, when full publicity will be within reach of all whom it may concern. I assure you that I am a very bad secret keeper, and it is a relief to my mind to know that the publication of the information you all seek is so imminent.

In reply to Mr. Moulton as to what should be done when a lamp fails, I can best explain this by ocular demonstration. [A specimen lamp was on the table before Mr. Swan, who released the large glass globe used for subduing the brilliancy of the light, and then simply lifted it out of its socket in the brass pedestal, exactly as a candle would be slid out of a candlestick, and then replaced it with equal facility by sliding another lamp down the pedestal to its position, when it was instantaneously lighted, and then, by replacing the covering glass globe and securing it in the usual manner, the whole operation was complete.] To explain, I may say that if anything goes wrong, the lamp should be first taken out of its socket, and another lamp fetched from the store closet and put in its place, as I have just shown you. That is an operation which I do not think demands an amount of skill at all beyond the capacity of an ordinary domestic servant.

As to the economy of my lamps (and here I will reply broadly to Mr. Siemens and Professor Ayrton), I accept the statement that it has been ascertained, not once, but many times, that one horse-power will develop, by means of the arc light, fully a thousand candle-power, and therefore I do not pretend to say that my lamp compares in economy with the arc light, where a very powerful and concentrated light is desired. I have not spoken of getting more than 150 candle-light from one horse-power. I may be able to get more, but at present I am satisfied with that result, because I can divide it as I please, and I think with Mr. Varley that small lights are so much more generally useful than very large ones, as to be preferable, even if obtained at a slightly greater cost light for light It is much more economical to have just as much light as you want than ten or a hundred times more than you want, even though the cost of the larger light may be less in proportion to its quantity than the smaller light. The fact is that the two things are quite distinct. The arc light is unquestionably more economical where it is applicable, but there are cases, and they are very numerous, where small lights are more useful than large ones; and considering the suitability for ordinary purposes of such small light centres as can be produced by means of my lamps, I think that we may be well satisfied with

the amount of light we get, although it be less than we could get in a concentrated form by means of the arc.

The arc light is something like a white elephant, for a great many purposes. When you have obtained it you do not know what to do with it. It is an admirable light for lighthouses, railway stations, and large open out-door spaces; but I think there can be no question that for the lighting of houses and of shops (and these are the chief purposes for which artificial light is required) small lights rather than large ones provide the desideratum, and, as I have said, are worth their extra cost. As to the large lights at the British Museum, no doubt the reading-room is one of the spaces suitable for lighting by means of the arc light, and therefore the installation at that institution has been pronounced a success. Probably the number of rooms so suitable is limited.

It is a great pleasure to me to hear the observations made by Professor Tyndall. I had not been bold enough to imagine that the electric light might be taken into the dangerous parts of a coal mine: I felt that it was a matter requiring very great care and caution, and that perhaps we were in danger of hoping too much in that direction. But to hear Professor Tyndall (who has, I know, gone minutely into the question of the application of the electric light to coal-mining) say that he thinks it is not impracticable to make use of such lights, if properly guarded, in the dangerous parts of coal mines, affords me very great pleasure.

To confirm what has been stated as to the economy of the light over gas, it is a very clear demonstration to this end to find that it is possible by means of a gas engine, an electric generator, and my lamp, to get absolutely more light from a given quantity of gas, than would be obtained with the same quantity of gas burned in the ordinary way for producing gaslight. I do not conceive that a clearer demonstration of the economy of the electric light over gas can be given than the fact stated.

Sir Charles T. Bright: I am sure you will all accord a hearty vote of thanks to Mr. Swan for his paper. The silence with which the paper was listened to, and the applause with which the lighting of his lamps was met, showed the feeling of this Society, which must be shared in by all present. The

spirit of prophecy was indulged in for a moment by Mr. Swan. I will take the same line for a moment, too, and venture to express my belief that in five years we shall have the electric light in familiar use in our houses. We have it already in our streets, and I hope and think, also, that by the time I have mentioned we shall have electricity used for some of the ordinary purposes of tramway and railway conveyance.

I beg to propose the hearty vote of thanks of this meeting to Mr. Swan.

Mr. LATIMER CLARK seconded the proposition.

The PRESIDENT: Gentlemen,—It has been proposed and seconded that a hearty vote of thanks be accorded to Mr. Swan for the very clear and beautiful paper he has given us to-night. I only wish I had a quarter of an hour before me to say what I have to say on the subject, but I will relieve you and satisfy myself by simply expressing my great obligation to Mr. Swan for the very great trouble and kindness with which he responded to my request to come here and give this admirable and excellent illustration which he has done to-night. For my part, I very much prefer prophesying when I know. I do not like to indulge in anticipation like Sir Charles Bright. I am one of those who up to the present moment have not been satisfied with the solution of the problem of the subdivision of the electric light, and I am also one of those who are not quite satisfied with the utility of the incandescent *versus* the arc light. But I very clearly see that there are occasions or circumstances, such as have been alluded to by Mr. Moulton and Mr. Swan, when certainly such a brilliant light as that now before us will be a great advantage to those who indulge in luxuries. It is as a luxury at present that we must regard this light. It is not without a sphere of danger. I do not agree with (and I am very sorry to differ from one from whom I have gained so much instruction, and whose opinion I value so much) Professor Tyndall that the electric light is adapted for coal mines. It is quite impossible to carry powerful currents, such as those now passing through the wires before us, through a colliery laden with gases, without some chance of danger. Even in the small experience of telegraphy, fire and danger have occurred from

comparatively minute currents. Fatal cases have occurred caused by incautious and foolish handling of lighting currents, or of the exposed parts of an electric lamp. There was a case recently on board the Livadia, another at Birmingham, and very nearly another on the Thames Embankment. Several cases of serious injury have occurred; and if the subdivision of the electric light is to be carried out as prophesied by Sir Charles Bright and Mr. Swan, it can only be, with our present knowledge, by the employment of such very high electro-motive forces as must necessarily be dangerous to human life. Certainly in the case of coal mines the employment of the electric light must be accompanied by dangers in another direction. However, the question before us to-night is the progress of the electric light. We have made a distinct step forward. We have passed through the various stages of electric lighting, from the first flickering, whizzing, noisy, erratic, and troublesome lamp which was attempted to be shown in this room, till we have this beautiful steady light which is now before us, showing what a satisfactory advance has been made. I have therefore great pleasure in offering to you for acceptation the proposal of Sir Charles Bright, seconded by Mr. Latimer Clark, that our most hearty thanks be given to Mr. Swan for bringing before us his latest achievements.

Carried with great applause.

A vote of thanks was also given to Mr. Crompton, Mr. Ward, and Mr. Fleetwood for assistance rendered in making the exhibition so successful.

The PRESIDENT then declared the meeting adjourned until Wednesday, December 8th, when he proposed to bring forward the recent advances made in the reproduction of sound by radiant energy, and announced that at the same meeting, through the kindness of Professor Bell (who himself hoped to be present), the Photophone would be exhibited.

The Ninety-third Ordinary General Meeting of the Society was held on Wednesday evening, December 8th, 1880, at the Institution of Civil Engineers, 25, Great George Street, Westminster—Mr. W. H. PREECE, President, in the Chair.

The minutes of the previous meeting were read. It was announced that the following Associates had been transferred by the Council to the class of Members:—

Henrik G. C. Bohr.
Christian Dresing.

At the request of the President, Mr. Latimer Clark then occupied the chair.

The PRESIDENT: The hall of the Institution of Civil Engineers (in which we are now assembled) is not like the theatre of the Royal Institution or places of similar consequence prepared for experimental illustrations, or fitted with the numerous appliances requisite to bring before a large audience such effects as those I hope to bring before you to-night. The limited time in which we have been able to fix up the apparatus before you makes it very doubtful whether all the experiments will succeed, and therefore I ask your indulgence in case of failure in any attempts I may make to bring before you facts that have been recently brought out. I had strong fears, too, that the proceedings to-night would be very much of the effect that would be produced by the play of *Hamlet* being acted with the character of Hamlet left out; but I am happy to say that Professor Bell has travelled from Paris on purpose to be here, and although he is not present at the moment, I have no doubt he will be very shortly.

It has been my special good fortune to have brought before the Society of Telegraph Engineers the three great inventions that have characterised the last two years. First, I brought over the first absolute speaking Telephone; next I had the pleasure of bringing before an audience at the Royal Institution, and also before this Society, the Phonograph; and thirdly, it was my great fortune to have been the means of unearthing a philosopher, and bringing before this Society that most beautiful apparatus, the Microphone.

And as regards the latest invention, the Photophone, Professor Graham Bell, on a recent journey from Paris to London, paid me the great honour of asking me to bring it before the British public. It was my misfortune to be unable to do so, as matters of great and imperial consequences (and indirectly associated with Professor Bell) compelled me to devote my whole time and attention in another direction. Professor Bell has himself brought the subject of the Photophone before one or two scientific societies, and I trust he will himself relate his most recent discoveries to us to-night.

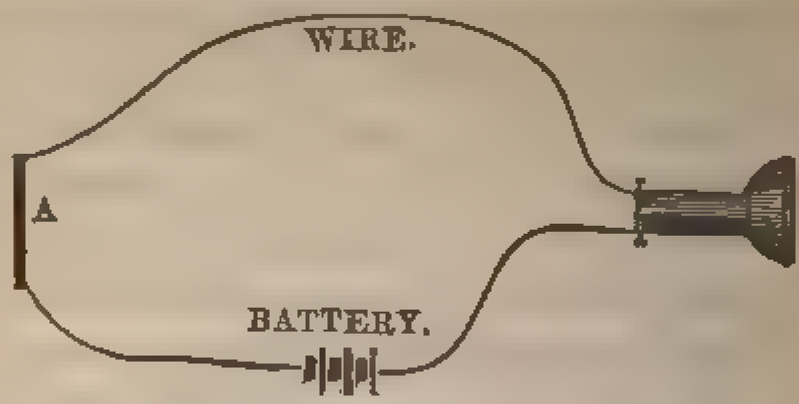
All those inventions I have enumerated are based on the reproduction of sound. Let us ask ourselves the question, What is sound? Sound is a sensation or physiological action that occurs upon the brain. Some mechanical disturbances affect the tympana of our ears, and there produce that which we call sound. Outside the mind sound cannot be said to exist; there it is sonorous vibration. It is an undulation, a disturbance in the molecules of matter that exist between the ear and the object emitting the vibrations. Hence wherever sound exists there must be a transmitter, or something producing the sonorous vibrations; secondly a medium conveying those so-called sonorous vibrations; and thirdly, a receiver, viz., the ear, which accepts or receives those vibrations and converts them into that sensation that we call sound. In most of the cases that I am about to bring before you, it is not so much a question of the transmission of sound as of its reproduction.

I will commence by calling your attention to the production of sound by sympathy. Whenever we throw that wonderful element called the atmosphere, by which we are surrounded, into sonorous vibrations, matter bathed by it receives, accepts, and answers back the vibrations given to it. For instance, here are two tuning forks. There is no visible connection between them, but you see that when Mr. Ladd throws one of them into vibration, and after a brief space of time stops it, the same sound is given out by the other. [Experiment shown.] That is sympathy. There is a case in which sound is actually transmitted. It is by what is known as Wheatstone's lyre. If a musical box were placed in a cellar the tunes would *not* be heard on account of distance and obstructive media, but if

a thin piece of pine projecting into this room were placed on it, and a tray or membrane be placed on the projecting wood, to act as a sounding board in this room, the tunes would be quite distinct. [No cellar being available the experiment was illustrated by means of a musical box being enclosed in another box, and then when a guitar was placed on the upright lath the tunes were distinctly heard.] The common method now of reproducing sound at a distance is by an instrument that some of us have heard of lately a great deal,—most of us perhaps too much,—and which has been called a "telephone." By means of this peculiar instrument (which, by-the-bye, performs all the functions of a telegraph), we are able to reproduce sounds in various degrees of efficiency. [Professor Bell now arrived, and was received with great applause.]

My present object, however, is to reproduce sounds by means of radiant energy. By radiant energy I mean those wonderful undulations that are known to pass through space from the sun to the earth, that convey to us sensations of light and heat, and the chemical actions that reproduce those astonishing photographic pictures. This radiant energy is conveyed to us from the sun by the vibrations of what is called the luminiferous ether, or, more frequently, by the simple name of ether. By the aid of Mr. Ladd and his electric light I hope to make clear to you what this radiant energy is. It is by the spectrum that I hope to do this. A glass prism breaks up into its component parts any of those rays of light that pass from the sun, or, as is more convenient, from our miniature sun, the electric light, on to the screen behind me. [Spectrum shown on screen.] That is one of the most beautiful objects produced by science, and one on which no one (whatever the extent of his knowledge) can fail to look without a certain amount of rapture. Beyond the colours appearing on the screen on the red side are other rays of low refrangibility, and which produce heat. Beyond the violet rays are ultra-violet rays of higher refrangibility, which, when caused to impinge on sulphate of quinine, become degraded into a lower form, and bring out light also. Here we have what I wish to call your attention to, viz., radiant energy in its component forms. In order to convert this radiant energy into sound, we have simply to produce by its aid

either a rapid intermittent action or a variation in the action of electric currents. Here is a diagram showing a telephone joined




up in circuit. As long as the electric current going through that telephone is continuous there is no disturbance produced, but if at point A an instrument be inserted capable of producing intermission in the flow of the current, or of producing variation in its strength with sufficient rapidity, sound will be produced. On the table is an instrument represented at point A on the diagram; it is a wheel-break. When the wheel is rotated currents pass whenever the contact-maker passes over the brass divisions (which are separated by ebonite), and a ticking sound is produced which varies according to the rapidity of the motion of the wheel; and when these ticks follow each other at a rate greater than 16 per second we get musical tones. [The wheel was turned with varying velocities, and corresponding high or low sounds were produced.] Those sounds are produced by periodic variation of the electric currents. If those currents followed each other so as to represent by their variation the variation in pitch, in loudness, and in form of those sounds produced by the human voice, we should be able to reproduce the articulation of human speech. All of you will recognise this as the principle of the telephone, and if either a telephone or microphone replaced the wheel-break, human speech would be reproduced. But we want the object at A (the wheel-break) to be affected by radiant energy. Radiant energy, when incident on the object A, must affect the currents of electricity passing through it so as to reproduce the sonorous vibrations. Can any substance be found in nature which is so sensitive to the action of radiant energy as to be affected in the way desired?

There is one, and at present only one, element or matter that

has at present been found so sensitive as to be attacked, as it were, or altered by the rapid impingement of the rays of radiant energy upon it. In February, 1873, we had the pleasure of hearing in this hall Mr. Willoughby Smith's discovery announced. He found that when a ray of light fell upon selenium the resistance or influence of that material to the electric current was altered. He took this curious substance, that is not a metal and yet is a metal, that is a kind of borderer flying from one side to the other and a very vagrant in its habits,—it is one of those things classed with phosphorus, tellurium, and sulphur,—and he found it offering such a considerable resistance to the electric current that he could use it for telegraphic purposes, and moreover he found that it was wonderfully sensitive to light. Mr. Willoughby Smith himself is here, and will reproduce the experiment, and will show us how the incidence of a ray of light upon a bar of selenium will alter its resistance. [The gas was turned down and Mr. Willoughby Smith requested extreme quietness during the experiment. A current of electricity was passed into selenium while in the dark, and no deflection appeared on the mirror galvanometer, but when a ray of light was brought to bear on the selenium its condition was altered and a deflection obtained.] Now the question arises as to whether that effect was due to the light rays, the heat rays, or the actinic rays of the radiant energy of which I have spoken. Many earnest workers have pursued this matter, notably Captain Sale, R.E., who found that no effect was produced at the outside limits of the spectrum: the greatest effect was produced among the red rays. That observation has not been confirmed, for Professor Bell and Professor Adams have both found that the maximum effect is really found in the greenish-yellow rays. Nevertheless, the observations of Captain Sale, Lord Rosse, Werner Siemens, and others who have since worked in the same direction, have unmistakably proved that this effect is one of light and not of heat or actinism. A pupil of mine in India was discussing this subject with a pupil of Werner Siemens. They read the announcement in a paper, and the pupil of Werner Siemens said, "Werner Siemens will find this out and make something of it." Dr. Werner Siemens did. He worked at the idea with tremendous energy, and found in

the first instance that which has been subsequently found by Professor Adams and others, that the variation of the resistance was as the square root of the illuminating power of the light. From this he proposed to make a photometer, but though he succeeded admirably in that object, it was not a reliable instrument, for the simple reason that the action of the light on selenium has been found to be extremely variable.

No two pieces of selenium will give the same effect; no two parts of the same selenium will give the same effects, and the same selenium will not give the same effects on two successive days; and this great variability has resulted in preventing selenium being utilised for photometric purposes. But Dr W. Siemens and his brother (who is so well known to all of us) then produced what is called a selenium eye. It might be a Siemens eye, though it does not look quite so far. It is made, as shown before you, of a black metallic ball with a selenium plate at the back portion, a lens in front, and closed "eyelids" in front of the lens. In front of the eye on the table is placed a galvanometer, which will record the electric effects. At the present moment a current is passing, similar to that in Mr. Smith's experiment. I now open the eye to the light, and the galvanometer is immediately deflected to ten degrees. I shut out the light and the deflection ceases. It is remarkable that there is a kind of sympathy or analogy between this "eye" and the human eye, for if we allowed the light to fall upon it too long, the eye would become fatigued and require sleep and rest to restore its vigour. Hence Dr. Siemens pointed out that, while selenium was extremely sensitive to light, it gave evidence of that peculiar phenomena called by mechanics and engineers "fatigue," which required time to restore it to its pristine vigour. Dr. Siemens also formed a very sensitive selenium cell, called "Siemens' grating" The term cell is misleading, and any one who would propose a better term would confer a boon upon investigators.

Having proved that the effect is due to light gives rise to the question, Does the light diminish the resistance of the selenium, or does it set up in the selenium a species of electrification or electro-motive force that produces a current of electricity in itself?

In other words, is the light on the selenium similar in its action to the effect of heat on a thermo-pile—to produce electricity or heat on gutta percha to reduce its resistance? If the effect is to produce electricity in the same direction as the original current, the effect is precisely the same as that of diminished resistance. In our practical tests in telegraphy we know that one of the greatest sources of error is due to the presence of earth currents and other electro-motive forces in our wires. It is quite possible in testing a submarine cable to get such results that may be ten miles or more out, and it is only by the introduction of checks and counterchecks, by tests for earth currents and foreign electro-motive forces, that those effects can be eliminated.

The question has been very carefully examined and worked out by Professor Adams, with the assistance of Mr. Day. Professor Adams has shown that there exists in selenium a photo-electric action. He has shown that when a ray of light falls upon a bar of selenium there is set up in that bar an electro-motive force which produces a current, the bar being, in fact, for the time converted into a small battery. I will ask Professor Adams to kindly repeat his experiment before us. [Professor Adams said he was afraid that the experiment could not be easily performed, as it required all the contrivances and quietness of a laboratory, but he repeated the fact that the result of his experiments proved that light falling on selenium produced an electric current without any battery at all being in circuit.]

Mr. Crookes' experiments have confirmed those of Professor Adams. Mr. Crookes has performed an experiment which, like that of Professor Adams, is also too delicate to be produced before an audience. But to show the care that the philosopher needs to exercise in watching his experiments, I will just mention that in one of Mr. Crookes' researches he had a radiometer, the vanes of which were coated on one side with chromic oxide and on the other with selenium. In May of one year he found that the radiometer rotated in one direction, while in October of the same year it rotated in the reverse direction. Mr. Crookes is one of those who, like most ardent workers, will not accept errors of observation as an explanation of anomalous results: an observation of that

kind means something, and he found that the only difference between the observation made in May and that made in October was that in the one case he used a *sperm* candle, and in the other case a *wax* candle. The sperm candle, which gave the whitest light, caused the selenium to retreat, and the wax candle, which gave the yellowest light, acted in a similar manner on the chromic oxide. That shows that some substances convert the rays of high refrangibility into heat, while others convert the rays of lower refrangibility into heat, selenium being one of the former class.

It not having been settled whether this effect was one of electro-motive force or of resistance, Mr. Sabine (whose paper is in the *Philosophical Magazine* for June, 1878) took two simple plates of selenium. Selenium is something like sealing wax, capable of being melted and moulded into any form, though it is a most expensive article. It melts at 217 deg. centigrade. Mr. Sabine took two plates of selenium and placed them in liquid (plain water) in a dark cell. A galvanometer was joined up in circuit. A ray of light exposed to one plate caused the galvanometer to be deflected in one direction. A similar operation on the other plate caused the deflection in reverse direction. That experiment proved that the action of light on the surface of selenium was to set up on its surface an electro-motive force. Mr. Sabine proved still further that thermometric heat acted in a similar manner to light. That was proved by a selenium cell surrounded by a tube through which warm water was passed.

Professor Minchin (of Cooper's Hill College), following in the steps of Becquerel and Grove, went into the subject more from its photographic aspect. He found the remarkable fact that if two clean plates of tin-foil are inserted in hard water, and light is admitted upon one plate, a strong electro-negative effect appears, which after a time (which is very variable) changes into an electro-positive effect.

We are fortunate in having nearly all the workers in this field of enquiry with us to-night. I am glad to say that Professor Minchin is among them. He has worked on this subject for a long time. He wrote an admirable paper (which was read before the British Association), and showed in it that the influence of

light was remarkable on eocine and on many emulsions used for photographic purposes. [Professor Minchin exhibited his cell, and showed the very high deflection produced after a brief exposure of one of the tin-foil plates to a magnesium light.] That is another proof that electro-motive force is produced on the plate by the incidence of light upon it.

There is a good deal of wild hypothesis frequently indulged in in enquiries of this kind, and when strange facts are brought before us we all try to account for them according to our lights; but in this particular case all theory must be mere conjecture, because in reality we know very little of the constitution of matter, and especially of such matter as selenium, which sometimes assumes a crystalline form, sometimes an amorphous form; at one time looking very much like a metal, and at another time most unlike a metal. Before this critical audience I will not venture any hypothesis as to the alteration of selenium under the influence of light. Its adaptability to producing sound must have occurred to most persons familiar with the theory of the telephone. Professor Bell, long before he handled selenium, expressed the idea in language used by the poet:

> "Such silence: one might hear a shadow fall
> Athwart the silence."

Mr. Willoughby Smith three or four days afterwards stated in this room that he had heard a ray of light fall on a bar of metal. I had worked at this also, because I saw the possibility of reproducing sound; but I failed. Those who work hard do not heed failures: they are accustomed to them. Failures always promote progress; and though one failure may lead to another failure, if you have health, time, and strength, you may depend upon it that a series of failures at last leads to success.

Professor Bell, with the assistance of his friend Mr. Sumner Tainter, attacked this subject. They saw the reason why we on this side of the Atlantic had failed. They increased the surface of selenium exposed to the action of the light; they eliminated that defect called "fatigue," and secured success by obtaining less resistance, a greater margin of change due to the incidence of light, and greater rapidity of action. Professor Bell's apparatus is

now before us. It has been especially designed for sunlight effects, and therefore I will not attempt to work with it to-night. But another earnest worker in the field, Mr. Shelford Bidwell, who sits modestly here, has produced a selenium cell both simple and cheap. In Professor Bell's arrangement the principle is to increase the surface. Mr Shelford Bidwell, following the same lines, has taken a small piece of mica, around which he has wound two copper wires at a short distance from each other. Selenium is then poured over this surface and allowed by a slow process to anneal. I ought to have mentioned earlier that selenium is found in two states—first, amorphous or vitreous, transparent and non-conducting; secondly, crystalline or metallic, opaque and conducting. These two stages pass into each other sometimes imperceptibly: if selenium is cooled rapidly it becomes amorphous, if cooled slowly, crystalline. I will now place Mr. Bidwell's cell in the position A on the diagram in place of the wheel-break. A ray of light is falling on the selenium, in front of which is a zinc disc which has been cut radially so as to cause a periodic incidence on the cell. The cell itself is in circuit with an Edison loud-speaking telephone, which will indicate the sounds transmitted. [The first experiment failed owing to a broken wire, but on a second attempt, sounds were heard from the telephone.] Those sounds were produced by the intermittent action of the ray of light on the selenium cell. It is most curious how frequently the poets anticipate practical men. Coleridge said,

> "Oh, the one life within us and abroad,
> Which meets all motion and becomes its soul—
> A light in sound, a sound-like power in light."

There he anticipated the reproduction of sound by the mere intermission of a ray of light.

I dare scarcely venture to tread on the ground of Professor Bell when he is himself here, but I will complete this branch of my subject before I give you the opportunity of hearing that gentleman himself. I will show you how light which can be converted into sound can be utilised for speaking purposes at a distance. In the first place, I pointed out in the early part of my remarks that to produce sonorous vibration we must have the

vibration of ponderable matter, and, as you all know, in the telephone and phonograph it is the vibration of discs that produce the representation of human speech. On the end of the table before you is a mirror made of microscopic glass: it is so thin and elastic that when I make a sound into it it vibrates, and, now the gas is turned down and a beam of light thrown on the mirror, you will see reflected on the screen the sonorous vibrations from my mouth as they appear in the mirror disc. The vibration of the disc throws the beam into undulations. That represents the simple transmitter used by Professor Bell. I will now cause a few diagrams to be thrown on the screen to illustrate the progress made in following up this subject.

1. Mr. Willoughby Smith's Selenium Cell.—A bar of selenium with platinum wires inserted at each end. It has been found that much variation occurs in the amount of contact between different metals. The contact between platinum and selenium is very bad, and represents a very great resistance. Copper and brass are very much better, and one of the principal merits of Professor Bell's cell is the fact that he uses brass instead of platinum. In the same way Mr. Shelford Bidwell's cell works well because he uses copper instead of platinum.

2. Dr. Siemens' "Grating."—This is made by a zigzag arrangement of two platinum wires running very closely together, and coated with selenium. Its size is about that of a threepenny-piece. Had Dr. Siemens increased the size, no doubt he would have obtained greater results.

3. Professor Bell's Arrangement.—This is made up of a series of brass and mica discs, representing very much that which is known to telegraphists as a condenser. The brass discs have the same diameter as the whole of the case: the mica separating discs are of a slightly smaller diameter, and between the two a small ring is left, which is filled with selenium, and the whole arrangement then becomes alternate surfaces of brass and selenium, insulated from each other by discs of mica. Professor Bell's arrangement consists of a hundred or more of these discs.

4. Here is the parabolic mirror which receives from the transmitting mirror the parallel rays of light, and concentrates them on

the selenium disc in the centre, where they produce the same effect as was shown in Mr. Bidwell's cell.

5. This figure shows the photophone complete as arranged to be worked by solar light. The solar rays are incident on one mirror, and are reflected from the lens through a water trough on to another mirror, and there thrown into vibration by the voice of the speaker, when they become undulatory in proportion to the sonorous vibrations of the voice. Those are representations of all the paraphernalia used by Professor Bell in his photophone. The actual apparatus is on the table before you, but, as I have said, it is not shown in operation on account of being constructed for solar action. Professor Bell carried out his experiments in Washington in the most beautiful atmosphere, clear sky, and brilliant sun, and with everything conducive to success, but had he relied upon a British sun for the purpose I am afraid he would have been dreadfully disappointed. It is perfectly true that Professor Bell has spoken through a distance of 230 yards. I have heard Mr. Bidwell speak through 30 or 40 feet with as great clearness as in the first telephones I heard. Yesterday, at the Royal Institution, I heard nursery rhymes with a clearness which was astounding. Mr. Bidwell took down a volume of Professor Clifford on dynamics, and read a paragraph giving a whole series of mathematical symbols, and I never lost a sound. The "s's" were perhaps not clear, but my imagination filled the hiatus. Therefore, notwithstanding the presence in this room of several sceptics, I have not the slightest hesitation in saying that distinct articulation can be obtained from this wonderful instrument, the photophone. Members will be at liberty to see and hear for themselves at the close of proceedings, for we have Mr. Bidwell's photophone joined up ready for use. It is worked by lime light instead of the electric light, on account of the latter being accompanied by a hissing which disturbs clear articulation.

The photophone is very old, though I am sorry to announce this before Professor Bell. There is nothing new under the sun. A certain poet said,

> "Memnon, ever gazing at the east,
> Waits till the arrows of the brightening dawn
> Smite into sound the silent lips of stone."

That was simply putting into the words of poetry the fact that one of those magnificent statues near Thebes at early dawn uttered a moaning wail. It was due, doubtless, to some rarifying effect of the rising sun on the pure air of the desert. I have heard a celebrated aeronaut say that at sunset he has heard a low wail come as it were from the horizon. No doubt that is the same effect as that experienced at Memnon. I remember reading a remark made by an old blind mathematician, who, when asked what was his conception of red light, said that "it reminded him always of the blast of a trumpet." Many of you perhaps have read that magnificent apostrophe to light in Milton's Paradise Lost, where he says,

"With the year
Seasons return, but not to me returns
Day, or the sweet approach of even or morn,
Or sight of vernal bloom, or summer's rose,
Or flocks, or herds, or human face divine;
But cloud instead, an ever-during dark
Surrounds me; from the cheerful ways of men
Cut off, * * * *
And wisdom at one entrance quite shut out."

It has been my pleasure on one occasion to have indicated to you how, with the aid of Professor Hughes' microphone, we might make the deaf hear. It has now been my pleasure to show how Professor Graham Bell has even made the blind to hear the light, and partially to reopen this entrance to wisdom; and I have left purposely in the background the greatest of Professor Bell's discoveries, the wonderful direct influence that light has upon matter itself, that you may have the pleasure and gratification of hearing Professor Bell himself.

Mr. Latimer Clark: Mr. Willoughby Smith, have you any observations to make?

Mr. Willougby Smith: I should like to have said a few words, but seeing the late hour of the evening, perhaps it would be keeping the members too long, and I will forego my remarks.

Professor Alexander Graham Bell: It gives me great pleasure to have the opportunity of saying a few words to you upon matters not touched upon by Mr. Preece, although I would

rather he had continued his discourse. It certainly is a very extraordinary sensation to hear a beam of sunlight laugh, cough, and sing, and talk to you with articulate words; but when you come to understand how these results are accomplished, you see that the operation is a very simple one. Mr. Preece has told us that the only substance yet known to us whose electrical resistance is affected by light is the substance selenium. When we look back upon the history of the discovery of this property, and realise that it was accidentally made while investigating the cause of an observed variability of conducting power, it seems difficult to believe that selenium *is* the only substance whose electrical resistance is affected by light; and the conviction grows that a persistent search will reveal other chemical elements or compounds possessing this property. Of course, the first substances to which we would turn would be the chemical neighbours of selenium—tellurium, sulphur, and phosphorus.

Tellurium, we know, in the hands of Professor Adams has yielded indications of reduced resistance when exposed to light like selenium, although experiments recently made in America, at Yale College, by Professor Wright, with tellurium, in the shape of a thin film deposited on glass, have yielded exactly contrary results, that is, the resistance has been *increased* by light instead of being reduced. In Washington, I have, in conjunction with my associate, Mr. Sumner Tainter, sought to utilise tellurium in photophonic experiments, but our results have as yet been simply negative. The difficulties in the way of investigating changes of resistance in tellurium are of the opposite nature to those encountered in studying similar changes in selenium. In the latter case the normal resistance is so great that refined methods have to be employed, and in the former case the resistance is so slight that equally delicate apparatus has to be employed. In our experiments Mr. Tainter and I have been accustomed to cut a spiral groove in a non-conducting surface, which groove was filled with tellurium. We thus obtained considerable surface and increased resistance, but we were unable to detect any change produced by light.

In the year 1839 George Knox announced that sulphur and

phosporus became conductors of electricity when fused, but the authority of Faraday, who placed these substances amongst those which were non-conductors, both in the solid and liquid condition, seems to have overshadowed this statement, for their non-conducting nature seems still to be accepted without question.

I do not know how the matter may be in regard to phosphorus, but in respect to sulphur the statement of Knox has been verified in Washington by Mr. Tainter and myself. We have found that the yellow liquid formed by melting flowers of sulphur is unmistakably a condition of conductivity, and that its conductivity increases with rise of temperature, until the liquid begins to change colour. The conductivity then ceases, and no amount of cooling or reheating seems to be able to restore its conducting power. But if you heat melted flowers of sulphur to the point of highest conductivity, and then cool slowly, the substance retains, in a solid condition, at ordinary temperatures, a percentage of its conducting power. This I look upon as the index that points the way to the utilisation of sulphur in place of selenium, although as yet we have failed to obtain indications of change of resistance produced by light.

We all know that when a current of electricity is passed round the coils of an electro-magnet, a molecular disturbance is produced in the iron core of the magnet, and that if the current is interrupted a large number of times per second, the rapid succession of molecular changes in the soft iron can be observed as sound by placing the ear in close contact with the iron core. A musical tone is heard, the pitch of which depends upon the rapidity of the interruption of the electrical current. While discussing this fact with Mr. Sumner Tainter, it occurred to us that a molecular disturbance, *however produced*, should in a similar way be audible as sound when rapidly intermitted. We know that a molecular disturbance was produced by the action of light upon crystalline selenium, and it therefore seemed likely that when this substance was exposed to an intermittent beam of light a musical tone might be perceived, without any electrical apparatus, by placing the ear in contact with the selenium. On performing the experiment with bars or masses of selenium, no sound was heard. We also listened to masses of iron and other substances under the action of an intermittent beam of

light, in the hope that we might be able to hear the succession of molecular disturbances produced by the ray, but without success. When the crystalline selenium was connected in circuit with a battery and telephone, very perceptible musical tones were produced from the telephone when the selenium was exposed to an intermittent beam of light. It was our custom to stop the sound by shading the selenium with the hand. But on using a piece of hard rubber or ebonite to shade the light, it was found that this apparently opaque substance did not entirely cut off the sound audible from the telephone in connection with the selenium. It seemed as if an invisible beam passed through the ebonite or hard rubber, impinged upon the selenium, and, being rendered intermittent by such a perforated wheel as you see here to-night, produced a musical tone in the telephone. This tone could be at once cut off by placing the hand in the path of the invisible beam.

When we cut off the beam of sunlight before reaching the first lens by interposing the hard rubber, an invisible beam passed through the lens, was brought to a focus at the perforated wheel, was rendered parallel by the next lens and projected along a room for several yards, and was then brought to a second focus by another lens upon a piece of selenium in circuit with a telephone and battery, and a musical tone was clearly produced. In this case the rotation of the wheel interrupted what was then an invisible beam. This seemed to indicate some peculiar property about hard rubber or ebonite, and bearing in mind the experiment of which I have told you, of listening directly to crystalline selenium and other substances, the idea occurred to listen to the hard rubber while an intermittent beam of sunlight was falling upon it. The result was extraordinary. A clear musical tone was immediately perceived. By using the sheet of hard rubber as a diaphragm, and listening through a hearing tube when the intermittent beam of light fell upon the diaphragm, the tone produced was immensely increased in volume. We then discussed the question whether this property belonged wholly to hard rubber, or whether it was due to the form in which the rubber was presented to the beam of light. In our earlier experiments we had used masses of crystalline selenium and other substances, but without

success, and it seems probable that the molecular disturbance produced by light in opaque bodies was chiefly a surface action, and that the vibration had to be mechanically transmitted through the mass of the substance in order to affect the ear, and that this might be the reason why better results followed when the substance was used in the shape of a thin diaphragm. We constructed a thin diaphragm of selenium, and on placing it in the apparatus we found a similar, though less audible effect produced. We then tried diaphragms of other substances. I have some forty or fifty of the original diaphragms used on exhibition here: they are of different materials, and *all* produced musical tones when under the influence of an intermittent beam of sunlight. All metals, paper, gutta percha, different kinds of wood, even leather, and in fact all the substances we have tried in the shape of thin diaphragms, have yielded sounds. One very interesting and suggestive peculiarity showed itself in these experiments, and that was the difference in the intensity of the sound audible from discs exactly similar in every respect but of different material. This suggests a wide field for investigation.

In discussing with Mr. Sumner Tainter the necessity of using a thin diaphgram, we were led to another form of the experiment still more interesting. If the effect of intermittent sunlight was to produce a surface vibration, we were evidently listening at the wrong side of the diaphgram. If we could have our ear in relation to the illuminated side, the sound should be louder. We therefore varied the experiment by throwing the light into the interior of a tube. By bringing the beam of light to a focus just on the interior of the tube, the rays diverging from the focal point struck the inside of the tube, and our ear was then in relation to the air with the illuminated surface. By throwing a beam of sunlight into this tube, a very clear musical tone was produced. We have tried substances of different kinds in tubular form, and from every substance so tried we have produced a musical tone. I may say that it is not even necessary to use an artificial tube, but on throwing the intermittent beam of sunlight into the ear itself, a musical tone can be produced. From the large number of experiments made by Mr. Sumner Tainter and myself, we have

felt ourselves justified in announcing this as a general property of all matter; but of course it is impossible to obtain all substances in the shape of tubes or diaphgrams; and another form of the experiment has occurred to me since coming to Europe, by which the generality of the phenomena may be tested. The sun fortunately came out for about an hour in Paris, and I was able to test the experiment. A transparent vessel, such as a glass test tube, may be taken, into which substances can be placed in any physical condition (solid, liquid, or gaseous). Connect the open mouth of the test tube with a hearing tube, and listen while you submit the contents of the test tube to the action of an intermittent beam of light. All the substances tried so far have given sounds, except chlorate of potash in a state of powder, plain water, and air. I have tried 30 or 40 different substances. Crystals of sulphate of copper gave out clear musical tones. A whole cigar placed in the test tube emitted a beautiful sound; and even tobacco-smoke puffed into the test tube yielded a sound. The best results obtained have been from little chips of wood inserted in the test tube. I have since had the opportunity of repeating these experiments with the electric light at the Royal Institution, and I think I was able to demonstate the existence of these sounds to the satisfaction of Professor Tyndall and one or two others who listened to them.

I will not take up your time any longer, but simply say that any questions that it is within my power to answer concerning the experiments made in America, I shall be happy to do my best to reply to.

Professor W. G. ADAMS: I will only make a few remarks on a point which has not been raised this evening, but one which is of considerable importance, the resistance of selenium and its changes of resistance, when it is in the state in which it is sensitive to light. In the paper by Mr. Day and myself, published in the *Philosophical Transactions* in 1876, is a table of resistances of selenium in which there are three pieces with resistances of 63, 53, and 55 ohms: these pieces are all very sensitive to light. I would also draw attention to the changes produced in the resistance of crystalline selenium while it sti

remains in the sensitive state. One piece, which in May, 1876, had a resistance of 1,525,000 ohms, was found in May, 1877, to have a resistance of 3,950 ohms. Another piece, which in May, 1876, had a resistance of 120,000 ohms, was found in May, 1877, to have a resistance of 1,123 ohms, and another in the same period had its resistance reduced from 7,600,000 ohms to 745 ohms. Another piece, with a resistance as low as 28 ohms, was still very sensitive to light. During the interval no experiments had been made with these pieces, but the change is similar to that produced by a slow process of annealing. I have also shown, as Professor Bell has stated, that selenium is not the only substance which is sensitive to light, and probably there are other substances which are affected in the same way as selenium and tellurium. It is also clear, as has been stated, that an electromotive force is set up in the selenium by the action of light, but when a battery current is passing through the selenium, the principal portion of the effect produced by exposure to light is the diminution of the electrical resistance, whereby a considerable variation of the strength of the battery current is produced. I will not trespass further on the indulgence of the Meeting, especially as Professor Bell has only himself had time to give us such a short account of his own investigations, so much interesting matter have we had brought before us this evening.

Professor TYNDALL: My scientific seniors are in the room, and I would rather hear their observations than listen to my own. With regard to the improbability of selenium being the only substance sensitive to light, I agree with Professor Bell in thinking it unlikely that selenium should differentiate itself in this respect from all other substances.

With regard to the beautiful experiments referred to by Professor Bell, and in connection with which he has mentioned my name, I can bear testimony to the fact that musical sounds were emitted by all the substances mentioned. Professor Bell is now at the outset of his labours in this direction, and stands in no need of suggestions to stimulate or guide him in his enquiries. But perhaps he will allow me to state the thoughts which occurred to me as I witnessed his experiments. Every beam of light is also a

beam of heat, and it occurred to me that the result obtained by Professor Bell might be due to the known action of heat producing sudden alterations of shape or volume. Zinc, for example, yields very good results. Now zinc has a very high co-efficient of expansion, and it is conceivable that the sounds emitted by zinc may be due to brisk alterations of temperature producing corresponding expansions and contractions, and therefore vibrations, in the zinc, rather than to the specific action of light.

When Professor Bell was good enough to show me his experiments, I happened to be myself experimenting on the action of vapours upon radiant heat. Old experiments had revealed, and new ones had confirmed the fact that, as regards the absorption of heat, there were vast differences between vapours. This is well illustrated by the deportment of bisulphide of carbon and of sulphuric ether, one of which is highly transparent, and the other highly opaque to radiant heat. It occurred to me that, if the action were due to the absorption of heat, we might possibly extract musical sounds from sulphuric ether vapour, whereas bisulphide of carbon vapour being transparent to heat rays, they would go through the vapour unabsorbed, and produce no sonorous effect. I think Professer Bell will bear me witness as to the result. We placed a quantity of sulphuric ether vapour in a test tube, and allowed an intermittent beam of light to strike upon the vapour far above the liquid, and we heard distinctly a musical tone of a pitch corresponding to the rapidity of the flashes. We then took the bisulphide of carbon vapour, and tried it in a similar manner, but neither Professor Bell nor myself could hear any trace of a musical sound. Professor Bell, as I have said, is on the threshold of his investigation, and it might be hasty to affirm that the results he has obtained are due entirely to heat. They may be composite results, due in part to light and in part to heat; but I submit that the experiments I have described with two vapours, equally transparent to light, warrant our asking whether the sounds heard in Professor Bell's experiments with discs were not due to the absorption of heat, which threw the bodies impinged upon by the intermittent beam into vibratory motion.

On Tuesday, the 7th of December, I had the pleasure of witnessing some of the experiments of Mr. Shelford Bidwell on a photophone invented by himself. Lenses only were used to concentrate the light, the photophone being a small rectangle instead of a cylinder. The effects obtained with this instrument far exceeded any with which I had been previously acquainted. By means of two telephones placed in the theatre of the Royal Institution, the words evoked by a photophone placed in a distant laboratory seemed as loud and as distinct as if they had been uttered close at hand.

A hearty vote of thanks was then passed by the meeting to the President for his instructive discourse, and for introducing those gentlemen who assisted and made observations on the subject.

The PRESIDENT: Gentlemen,—My thanks and your thanks are principally due to Professor Bell for having come from Paris on purpose to be here to-night. I would also say that I cannot conclude without asking you to express your thanks to those gentlemen who have so kindly assisted me in bringing before you the experiments shown. Mr. Shelford Bidwell has brought his apparatus, and worked most indefatigably; and Professor Adams, Mr. Willoughby Smith, Professor Minchin, Mr. Sabine, Mr. Stroh, Mr. Ladd, and others, have come forward with such kindness and alacrity that will always induce your President to bring before you any novel subject in science. I must also thank you for the attention you have given, and especially for the welcome bestowed on Professor Graham Bell to-night.

A ballot for new members will now take place, after which the meeting will be adjourned until Wednesday, December 22nd, 1880, which will be the Annual Meeting.

Elected:—

*As Foreign Members:*

Gustave Amiot.

T. D. Lockwood.

*As Members:*

Walter Twiss Glover.

K. L. Murray.

*As Associates:*

| | |
|---|---|
| George Brooks Almond. | George Holmes. |
| Thomas Hanby Brayshaw. | J. C. Kidd. |
| George Crabtree. | W. MacWhirter. |
| James Franklin. | Edwin Pierce. |
| Captain Walter Goodsall. | John Brewer Saunders, Jr. |

Lieut. George Alexander Keith Wisely, R.E.

*As Student:*

Montague James Scott.

A General Meeting of Members and the Ninth Annual General Meeting of the Society was held on Wednesday evening, December 22nd, 1880, at the Institution of Civil Engineers, 25, Great George Street, Westminster—Mr. W. H. PREECE, President, in the Chair.

The PRESIDENT: In the notice calling this meeting together it was stated that it was to be a General Meeting of members, to discuss the question of the alteration of the title of the Society and other matters, and that afterwards the Annual General Meeting would be held. I have to announce that it has been thought better to reverse this arrangement, and therefore, with your permission, we will resolve ourselves into the Annual General Meeting first. The ballot-box will remain open till half-past eight.

Mr. F. DESPOINTES and Mr. J. F. BETTS were appointed scrutineers.

The SECRETARY then read the minutes of last meeting, announced new candidates, and read the Report of the Council for 1880.

## REPORT OF THE COUNCIL TO THE ANNUAL GENERAL MEETING OF THE SOCIETY.

DECEMBER 22, 1880.

The Council have to report that the additions made to the various classes of Members during the year are as follows:—

| | |
|---|---|
| Foreign Members ... ... ... ... | 5 |
| Members ... ... ... ... ... | 8 |
| Associates ... ... ... ... ... | 46 |
| Students ... ... ... ... ... | 1 |
| | 60 |

In addition to these, 19 candidates have been announced for ballot at the first meeting held next month, viz., 11 as Foreign Members, 2 as Members, and 6 as Associates.

The losses on the other hand by deaths and resignations have amounted to 29.

The character of the Papers and Communications read during the Session has been fully up to the standard of interest and value hitherto attained, as will be acknowledged on reference to the subjoined list:—

| SUBJECT. | AUTHOR. |
|---|---|
| Telegraphy in New Zealand ... ... ... | A. T. MAGINNITY, Member. |
| The Durability of some Iron Wire ... ... | W. H. PREECE, President. |
| Morse Signalling by Magneto-Electric Currents ... ... ... ... ... | A. EDEN, Associate. |
| Compensating Induction in Parallel Circuits | E. H. WILSON, of Chicago. |
| Note on the Electro-Magnetic and the Electro-Static Induction from Wire to Wire in Telegraph Lines ... ... ... ... | Profs. W. E. AYRTON & JOHN PERRY, Members. |
| A Fault in the Construction of Differential Instruments... ... ... ... ... | J. B. STEARNS, Foreign Member. |
| Some Recent Improvements in Electric Light Apparatus ... ... ... ... | ALEXANDER SIEMENS, Member. |
| Note on some Effects produced by the Immersion of Steel and Iron Wires in Acidulated Water... ... ... ... | Prof. D. E. HUGHES, F.R.S., Member. |
| Note on Prof. Hughes's Communication ... | W. CHANDLER ROBERTS, F.R.S. |
| The Adhesion of Metals produced by Currents of Electricity ... ... ... | A. STROH, Member. |
| Note on the Burning of the Positive Carbons in Electric Arc ... ... ... ... | J. D. F. ANDREWS, Student. |
| On the Resistance of Galvanometers ... | OLIVER HEAVISIDE, Associate. |
| On the Determination of the Position of Faults in a Cable where two exist at the same time ... ... ... ... ... | C. HOCKIN, Member. |
| Testing by Received Currents ... ... | H. R. KEMPE, Member. |
| The Behaviour and Decay of Insulating Compounds used for Dielectric Purposes | W. H. PREECE, President. |
| A New Electrical Speed Indicator for Engines ... ... ... ... ... | H. R. KEMPE, Member. |
| An Improved Form of Wheatstone Receiver ... ... ... ... ... ... | J. WILLMOT, Associate. |
| A Decade in the History of English Telegraphy ... ... ... ... ... | E. GRAVES, Hon. Treasurer. |
| The Dynamo-Electric Current in its Application to Metallurgy, to Horticulture, and to the Transmission of Power ... | Dr. SIEMENS, F.R.S., Past President. |

| SUBJECT. | AUTHOR. |
| --- | --- |
| A New System of Subdividing the Electric Light... ... ... ... ... ... | J. W. SWAN, of Newcastle-on-Tyne. |
| The Photophone of Prof. Graham Bell and the Conversion of Radiant Energy into Sound ... ... ... ... ... | W. H. PREECE, President. |

Some of these papers, notably those on the "Electric Light," by Mr. Alexander Siemens and Mr. Swan, "On the application of the Dynamo-Electric Current to Metallurgy, Horticulture, and to the Transmission of Power," by Dr. C. W. Siemens, and on "Professor Graham Bell's Photophone and the Conversion of Radiant Energy into Sound," by the President, were brilliantly illustrated by experiments, and led to discussions of much importance.

In addition to these papers, and other valuable original communications, the Society's Journal for the current year includes abstracts from some of the most important contemporaneous periodicals (both English and Foreign) on electricity and magnetism, carefully collated under the able superintendence of Professor Ayrton as Chairman of the Editing Committee. The Council believe that this new feature in connection with the Journal will prove of much value to the Members, especially to those who have not the opportunity of referring to the scientific publications themselves, from which the extracts and abstracts are selected and prepared.

The completion and publication of the Ronalds Catalogue is a fit subject of congratulation. It has been a work of much labour and considerable expense, but has resulted in the production of a volume of immense value to every student of electricity, and the praises so abundantly bestowed upon it by the English and Foreign press are a sufficient proof that the materials collected at the cost of so much time and money by the compiler, the late Sir Francis Ronalds, have been worthily dealt with by the Editor, Mr. A. J. Frost, the Society's Librarian. The Council strongly recommend those Members who have not yet applied for a copy of this important work to do so at once, as there is reason to believe that a considerable demand will shortly be made upon the remaining stock by foreign libraries and booksellers.

A still greater subject for congratulation is afforded by the completion of the binding of the books and pamphlets comprising the Library. This work, including the classification and formation

into volumes of the immense number of pamphlets, has been carried out under the superintendence of the Society's Librarian, and has met with the entire approval of the Trustees of the Ronalds Trust, whom the President and Council had the pleasure of welcoming upon the occasion of the inaugural opening of the Library on the 24th of last month, when they endorsed upon the trust-deed a minute recording that the Society had up to that time satisfactorily complied with the requirements of the Trust.

A very complete reference catalogue has been prepared by Mr. Frost, and Members having occasion to make use of the Library will find the utmost facility is thus afforded for referring to any particular work which it contains.

The Council have arranged that the Library shall be opened on and after the 1st of January next, under the following regulations:

1. The Society's Library is open to members of all Scientific Bodies, and (on application to the Librarian) to the Public generally.
2. The Library is open daily between the hours of 11.0 a.m. and 8.0 p.m., except on Thursdays and on Saturdays, when it closes at 2.0 p.m.
3. The Library will be closed from the 15th August to the 15th September—both days inclusive—and on all Public Holidays.
4. The Library is to be used for reference only, and no Books, Pamphlets, Journals, or Patent Specifications are allowed to be taken from the Library.
5. All works are to be returned to the Librarian or his Assistant, and are *not* to be replaced on the shelves by the readers.

The President of the Royal Society, Dr. Spottiswoode, thus refers to the Library and the Catalogue, in his Address delivered last month :—

"I am not sure how far the fact is known to the Fellows of the Royal Society that the Society of Telegraph Engineers has thrown open to the scientific world a remarkable collection of books on electrical science, collected by our late Fellow, Sir Francis Ronalds, and bequeathed by him to that Society. The catalogue, compiled by the collector, is a monument of concentrated and well-directed labour."

The Council have arranged to place at the disposal of the Library Committee the sum of £50 per annum, towards the expense of providing an attendant in the Library during the evenings, and for the purchase of modern works on electricity and magnetism, &c.; but as this sum will only allow of a limited number of new books being acquired, the Council trust that Members will use their best endeavours to obtain from authors with whom they may be acquainted the presentation of their works to the Society and Library.

The following is the Librarian's Report on the additions made during the year by presentation and by purchase:—

SOCIETY OF TELEGRAPH ENGINEERS,
4, BROAD SANCTUARY, S.W., LONDON,
*December* 20, 1880.

The Secretary,
Society of Telegraph Engineers,
4, Broad Sanctuary, S.W.

SIR,

I beg to hand you for the information of the Council, a list of the works which have been added to the Library during the past year. The list does not include the large number of periodicals which are regularly received in exchange for the Journal of the Society.

I have made arrangements by which the Patent Specifications, which are presented to the Society by Her Majesty's Commissioners of Patents, will, as they are published, be in the Library for the use of the Members every week, so that every specification relating to electricity, magnetism, &c., will be available for reference as quickly as possible after the date of its publication.

I am, Sir,
Yours faithfully,
**A. J. FROST,**
*Librarian.*

## LIST OF BOOKS ADDED TO THE LIBRARY DURING THE YEAR 1880.

**Adams**, Prof. W. G., F.R.S. On the action of Light on Tellurium and Selenium. 8vo. 7 pp. (*Proc. Roy. Soc., No.* 166.) *London*, 1876
Presented by the Author.

**Adams**, W. G., and **Day**, R. E. The action of Light on Selenium. 4to. 37 pp. (*Phil. Trans.*) *London*, 1876
Presented by Prof. W. G. Adams, F.R.S.

**Airy**, Sir G. B., K.C.B. Report of the Astronomer-Royal to the Board of Visitors of the Royal Observatory. 4to. 21 pp. *London*, 1880
Presented by the Author.

**Anderson**, R. Lightning Conductors; their history, nature, and mode of application. 8vo. 256 pp. *London*, 1879
Presented by the Author.

**Beechey**, Fredk. S. Electro-Telegraphy. 8vo. 126 pp. *London*, 1876
Purchased.

**Clark**, Latimer, and **Sabine**, Robert. Electrical Tables and Formulæ. 8vo. 285 pp. *London*, 1871
Presented by Latimer Clark, Esq., M.I.C.E. (*Past President*).

**Clausius**, R. Die Mechanische Behandlung der Electricität. 8vo. 352 pp. *Braunschweig*, 1879
Purchased.

**Du Moncel**, Le Comte Th. The Telephone, the Microphone, and the Phonograph. 8vo. 363 pp. Plates. *London*, 1879
Purchased.

——— L'Eclairage Électrique. 8vo. 364 pp. Plates. 2nd Edit. *Paris*, 1880
Purchased.

——— Notice sur la vie et le travaux de M. Gaugain. 8vo. 92 pp. *Caen*, 1880
Presented by the Author.

**Ellis**, Wm. On the relation between the Diurnal Range of Magnetic Declination and Horizontal Force . . . 4to. 20 pp. (*Phil. Trans.*) *London*, 1880
Presented by the Author.

**Encyclopædia Britannica.** Part 29. Containing articles on Electricity, Electrolysis, Electro-Metallurgy and Electrometer. 4to. Plates. *Edinburgh*, 1880
Purchased.

**Evans**, Capt., F.R.S., and **Smith**, A., LL.D., F.R.S. Admiralty Manual for the Deviation of the Compass. 8vo. 199 pp. Plates. 4th Edit. *London*, 1874
Purchased.

**Galante**, Don José. Manual de Mediciones Elèctricas. 8vo. 657 pp. *Sevilla*, 1880
Presented by the Author.

**Gibson**, J. C., and **Barclay**, T. Measurements of Specific Inductive Capacity of Dielectrics. 4to. 11 pp. Plates. (*Phil. Trans.*) *London*, 1871
Presented by Sir Wm. Thomson, LL.D., F.R.S. (*Past President*).

**Gordon**, J. E. H. A Practical Treatise on Electricity and Magnetism. 2 Vols. 8vo. 323 and 295 pp. Plates. *London*, 1880
Purchased.

**Haggerston**, W. J. Catalogue of Books of the Newcastle-upon-Tyne Public Libraries. 8vo. 329 pp. *Newcastle-on-Tyne*, 1880
Exchanged.

**Higgs**, Paget, LL.D. Electric Transmission of Power; its Present Position and Advantages. 8vo. 87 pp. Plates. *London*, 1879
Purchased.

**Higgs,** Paget, L.L.D. The Electric Light in its Practical Application. 8vo. 240 pp. Plates. *London*, 1879
Purchased.

**Hoffmeyer,** A. Etude sur les Témpêtes de l'Atlantique Septentrional et projet d'un Service Télégraphique International relatif à cet Océan. 4to. 45 pp. *Copenhagen*, 1880
Presented by J. L. Madsen, Esq.

**Japan.** Japanese text of the London Convention, 1879. 8vo. 1879
Presented by H. G. Erichsen, Esq.

**Jones,** Jno. The Sun a Magnet. 8vo. 44 pp. Plates. *Dundee*, 1880
Purchased.

——— Undulation of the Sun's Magnetic Nucleus. Sm. 8vo. 24 pp. *Dundee*, 1880
Presented by the Author.

**Knapman,** J. W. Catalogue of the Library of the Pharmaceutical Society. 8vo. 445 pp. *London*, 1880
From the Society. By Exchange.

**Lemon,** Dr. Report on the New Zealand Telegraphs. Sm. fol. 18 pp. Plates. *New Zealand*, 1880
Presented by the Author.

**Loring,** A. E. A Handbook of the Electro-Magnetic Telegraph. 12mo. 98 pp. Plates. *New York*, 1878
Purchased.

**M'Kichan,** Dugald. Determination of the Number of Electrostative Units in the Electro-Magnetic Unit. 4to. 19 pp. (*Phil. Trans.*) *London*, 1873
Presented by Sir Wm. Thomson, LL.D., F.R.S. (*Past President*).

**Maxwell,** J. Clerk, F.R.S. The Electrical Researches of the Hon. Hy. Cavendish, F.R.S., written between 1771 and 1781. Edited from the original documents in the possession of the Duke of Devonshire, K.G. 8vo. 454 pp. *Cambridge*, 1879.
Purchased.

**Mercadier,** M. E. Traité Élémentaire de Télégraphie Électrique. 8vo. 261 pp. Plates. *Paris*, 1880
Purchased.

**Meteorological Society.** Quarterly Journal of. 8vo. *London*, 1880
Presented by the Society.

**Overend,** Jas. Elementary Experiments in Electricity and Magnetism. 8vo. 76 pp. *Edinburgh*, 1879
Purchased.

**Petrushevsky,** F. Experimental and Practical Electricity, Magnetism, and Galvanism. La. 8vo. 2 vols. (1 vol. plates). (*Russian*). *St. Petersburg*, 1876
Presented by General v. Luders.

**Philosophical Society of Adelaide,** South Australia. Transactions and Proceedings, Reports of, for 1878–79. 8vo. 140 pp. *Adelaide*, 1879
Presented by the Society.

**Philosophical Society of Washington.** Bulletin of the. 3 vols. 8vo. *Washington*, 1874–80
Presented by the Society.

**Pissarewsky.** N. Handbook of the construction of Overground Lines. La. 8vo. 276 pp. (*Russian.*) *St. Petersburg*, 1878
Presented by the Author.

**Prescott,** G. B. The Speaking Telephone, Electric Light, and other recent Electrical Inventions. 8vo. 616 pp. Plates. *New York*, 1879
Purchased.

**Pope,** Frank L. Modern practice of the Electric Telegraph; a handbook for Electricians and Operators. 8vo. 160 pp. Plates. 10th Edition. *New York*, 1877
Purchased

**Rochnevsky,** C. The Telegraph and its application in time of War. 8vo. 332 pp. (*Russian*) *St. Petersburg*, 1872
Presented by General v. Luders.

**Royal Observatory,** Greenwich. Results of the Magnetical and Meteorological Observations . . . the year 1878. 4to. *London*, 1880
Presented by Sir G. B. Airy, K.C.B., F.R.S. (*Hon. Member*).

**Russian Telegraphs.** The Statistical Report of the Russian Telegraphs for the year 1877. La. 8vo. 78 & 35 pp. Plates and tables.
*St. Petersburg*, 1879
Presented by General v. Luders.

**Shoolbred,** J. N. Electric Lighting and its practical application, with results from existing examples. 8vo. 108 pp. Plates. *London*, 1879
Purchased.

**Siemens,** C. W., D.C.L., F.R.S. On the Smoke Question. 8vo. 16 pp. Plates.
*London*, 1880
Presented by the Author.

**Smith,** Willoughby. The working of long Submarine Cables. 8vo. 45 pp. (*Reprinted from Journal of Society of Telegraph Engineers.*)
*London*, 1880
Presented by the Author.

**Smithsonian Institution.** Annual Report of the Board of Regents for 1878. 8vo. 375 pp. *Washington*, 1879
Presented by the Institution.

**Society of Engineers.** Transactions for 1879. 8vo. 221 pp. *London*, 1880
From the Society by Exchange.

**Sprague,** J. T. Electricity: its Theory, Sources, and Applications. 8vo. 384 pp. *London*, 1875
Purchased.

**Thomson,** Sir Wm., LL.D., F.R.S. Effects of Stress on Magnetization. 8vo. (*Proc. Roy. Soc.*) *London*, 1875
Presented by the Author.

——— Effects of Stress on Magnetization. 8vo. 21 pp. (*Phil. Trans.*)
*London*, 1875
Presented by the Author.

——— Effects of Stress on the Magnetization of Iron, Nickel, and Cobalt. 4to. 36 pp. (*Phil. Trans.*) *London*, 1879
Presented by the Author.

**University College,** London. General Library Catalogue. 8vo. 3 Vols.
*London*, 1879
Presented by the College.

——— Calendar. Session 1880-81. 8vo. 335 and 87 pp. *London*, 1880
Presented by the College.

**Urquhart,** J. W. Electroplating. A practical Handbook, including the Practice of Electrotyping. 8vo. 216 pp. Plates. *London*, 1880
Purchased.

Electric Light; its Production and Use. Edited by F. C. Webb, M.I.C.E., M.S.T.E. 8vo. 290 pp. Plates. *London*, 1880
Purchased.

**Weinhold,** Adolf F. Introduction to Experimental Physics; Theoretical and Practical. Translated and edited by B. Loewy, F.R.A.S.; with a Preface by G. C. Foster, F.R.S. 8vo. 839 pp. *London*, 1875
Purchased.

**Woskresensky,** W. The Hughes Printing Instrument. 8vo. 94 pp. (*Russian*). *St. Petersburg*, 1879
Presented by General v. Luders.

A. J. FROST,
*Librarian.*

The adoption of a Universal Wire Gauge, upon which a committee of the Society reported last year, is still under the consideration of the Board of Trade, who have recently applied to the principal wire manufacturers for an expression of their views upon the matter, and there is reason to hope, therefore, that some satisfactory solution of this important subject may soon be brought to a practical issue.

In the last Annual Report reference was made to the intended application of the Society for the grant of a Royal Charter of Incorporation, it being one of the conditions of the Ronalds Trust that such application should be made. A petition was accordingly presented this year, but since the passing of the "Companies Act, 1867," which affords to Associations such as our Society a simple means of incorporation, the privilege of a Royal Charter has been so rarely bestowed, that the Members will scarcely be surprised to learn that the efforts of your Council, in which they were greatly aided by the advice and assistance of your Honorary Solicitor and the Honorary Secretary, proved unavailing. It is therefore their intention to take the necessary steps for obtaining the Incorporation of the Society under the Act above referred to.

The Report of last year also referred to an intended proposal to make some addition or alteration to the existing title of the Society with the view of indicating more clearly that its Members are not confined to those who follow the profession of Telegraph Engineering, and that its mission is to assist in the advancement and development of *every* branch of Electrical Science.

The question of what the alteration or addition should be, has been most carefully and anxiously considered by your Council, and after mature deliberation they have come to the conclusion that the object in view would be attained by adding to the present title the words "and of Electricians."

Although under the Rules of the Society this question can only be decided by the votes of Members, the Council desired before finally submitting any proposal to a meeting to ascertain as far as possible the views of all three classes of Members, and a circular was accordingly issued inviting an expression of assent to or

dissent from the proposition, and the result has been that out of 353 replies received, only 17 express dissent, while 8 merely suggest various alternative alterations, every one of which it may be stated had already been fully considered by the Council.

As the matter is to be brought before a General Meeting of Members this evening it is unnecessary for the Council to say more than that they have most carefully weighed the merits of every possible form of alteration which would meet the required end without prejudicing the original title under which the Society has attained its present satisfactory position among scientific bodies, and they leave the final decision to the members themselves.

As the Institution of Civil Engineers, to whom we are so much indebted for the use of their theatre, hold their meetings on Tuesdays, only one day is left for fitting up apparatus and making the necessary arrangements for our own meetings, and this has frequently been found most inconvenient. In order to obviate the difficulty, the Council have obtained the sanction of the Institution to our holding our Meetings in future on Thursdays instead of on Wednesdays.

The Council have pleasure in reporting that the financial position of the Society is now more satisfactory than has been the case for some years past.

Although in the general interest of the Society, and in order to augment the usefulness of its operations, an increased outlay under some heads has been sanctioned, the vigilance exercised by your Honorary Treasurer over the general expenditure has enabled the Finance Committee to repay in less than two years, instead of seven, as originally provided, the whole of the loan granted to the Society by a few of its members in February, 1879, and the Society will, therefore, commence the new year entirely free from debt.

## STATEMENT OF RECEIPTS AND EXPENDITURE FOR THE YEAR 1880.

### RECEIPTS.

| | £ | s. | d. | £ | s. | d. |
|---|---|---|---|---|---|---|
| To Balance, December 31st, 1879 ... ... ... | | | | 844 | 17 | 8 |
| " Subscriptions paid up for previous years ... | | | | 228 | 12 | 6 |
| " Subscriptions for the year 1880 ... ... | | | | 990 | 10 | 0 |
| " Subscriptions paid in advance ... ... ... | | | | 81 | 17 | 0 |
| " Entrance Fees due 1879 ... ... ... | 10 | 10 | 0 | | | |
| " Entrance Fees due 1880 ... ... ... | 48 | 18 | 0 | | | |
| | | | | 59 | 8 | 0 |
| " Life Compositions... ... ... ... ... | | | | 72 | 0 | 0 |
| " Publishing Fund ... ... ... ... ... | | | | 9 | 8 | 2 |
| " Sale of Journals ... ... ... ... ... | | | | 109 | 8 | 0 |
| " Advertisements in Journal ... ... ... | | | | 10 | 13 | 6 |
| " Sale of Ronalds Catalogue ... ... | 129 | 10 | 10 | | | |
| including charge for Postage and Binding ... ... ... ... | 16 | 16 | 1 | | | |
| | | | | 146 | 6 | 11 |
| " Profit on exchange of Foreign remittances ... | | | | 1 | 19 | 0 |
| " Interest on Premium Fund ... ... ... | | | | 0 | 9 | 11 |
| | | | | £1,986 | 2 | 8 |

### EXPENDITURE.

| | £ | s. | d. | £ | s. | d. | £ | s. | d. |
|---|---|---|---|---|---|---|---|---|---|
| By Salaries and Clerical Assistance ... | | | | 309 | 4 | 11 | | | |
| " Shorthand Reporter ... ... ... | | | | 21 | 0 | 0 | | | |
| " Attendance, Refreshments, and other Expenses at Meetings ... ... | | | | 89 | 15 | 4 | | | |
| " Printing and Illustrating Journal and cost of Abstracts ... ... | 260 | 13 | 4 | | | | | | |
| " General Printing and Stationery ... ... | 53 | 12 | 9 | | | | | | |
| | | | | 314 | 6 | 1 | | | |
| " Printing Ronalds Catalogue and binding same ... ... ... ... | | | | 163 | 3 | 6 | | | |
| " Sundry Expenses connected with maintenance of Library and Paintings, including Insurance ... ... | | | | 17 | 10 | 0 | | | |
| " Purchase of Books ... ... ... | | | | 8 | 4 | 0 | | | |
| " Rent, Gas, and Firing ... ... ... | | | | 184 | 11 | 6 | | | |
| " Postage, Cost of forwarding Journals and Ronalds Catalogue, and general Office Expenses ... ... ... | | | | 126 | 3 | 5 | | | |
| " Expenses of Local Honorary Secretaries | | | | 1 | 8 | 8 | | | |
| " Bank Charges on Foreign and Provincial drafts ... ... ... ... ... | | | | 0 | 11 | 11 | | | |
| " Interest on Loan ... ... ... ... ... | | | | 14 | 17 | 5 | | | |
| | | | | | | | 1,901 | 5 | 9 |
| " Repayment of Balance of Special Loan ... | | | | | | | 450 | 0 | 0 |
| " Purchase of £100 4 per cent. India Stock (Premium Fund) ... ... ... | | | | | | | 102 | 15 | 0 |
| " Balance in hands of Bankers ... ... | | | | 225 | 12 | 7 | | | |
| " Balance in hands of Secretary... ... | | | | 6 | 9 | 4 | | | |
| | | | | | | | 232 | 1 | 11 |
| | | | | | | | £1,986 | 2 | 8 |

We have examined the above Account, with the Vouchers and Books of the Society, and find it to be correct, leaving in the hands of the Bankers Two Hundred and Twenty-five Pounds, Twelve Shillings and Sevenpence.

J. WAGSTAFF BLUNDELL, } Auditors.
FRED. CHAS. DANVERS, }

## BINDING ACCOUNT (Late Conference Reception Fund Account).

| | £ | s. | d. | | £ | s. | d. |
|---|---|---|---|---|---|---|---|
| To Balance, December 31st, 1879 ... ... ... | 151 | 2 | 1 | By further Amount paid for binding ... ... | 117 | 18 | 8 |
| | | | | " " " Clerical Assistance in preparing Books and Pamphlets for binding | 12 | 4 | 4 |
| | | | | " Balance in the hands of Bankers ... ... | 21 | 4 | 1 |
| | £151 | 2 | 1 | | £151 | 2 | 1 |

We have examined the above Account, with the Vouchers and Cash Books, and find it to be correct, leaving in the hands of the Bankers Twenty-one Pounds, Four Shillings and One Penny.

J. WAGSTAFF BLUNDELL,
FRED. CHAS. DANVERS, } *Auditors.*

## ESTIMATE OF ASSETS AND LIABILITIES ON THE 31st OF DECEMBER, 1880.

### ASSETS.

| | £ s. d. | £ s. d. |
|---|---|---|
| Balance in hands of Bankers ... ... | 225 12 7 | |
| " " Secretary ... ... | 6 9 4 | |
| | | 232 1 11 |
| Unpaid Subscriptions, estimated amount realisable | | 184 11 0 |
| Journals in stock " " " | | 631 12 0 |
| Ronalds Catalogues " " " | | 200 0 0 |
| Estimated value of Furniture, viz., as per last Balance Sheet ... ... | 180 0 0 | |
| Less depreciation ... ... ... | 18 0 0 | |
| | | 162 0 0 |
| Objects presented to the Society (valued at £225, but this sum is not commercially realisable) | | |
| | | £1,410 4 11 |

### LIABILITIES.

| | £ s. d. |
|---|---|
| Subscriptions received in advance ... ... ... | 31 17 0 |
| Balance of Premium Fund uninvested ... ... | 2 14 11 |
| (There are no other Liabilities) | |
| Balance Cr. ... ... ... ... ... ... | 1,375 13 0 |
| | £1,410 4 11 |

E. GRAVES, *Honorary Treasurer.*
F. H. WEBB, *Secretary.*

Mr. E. B. BRIGHT proposed that the report of the Council be adopted, and said that it was a very able report. As far as the proceedings were concerned, he was quite sure that the work done by the Society in bringing forward so many matters of the highest interest in science before the world, in the year just concluded, must be generally appreciated.

Mr. G. G. NEWMAN seconded the proposition.

Mr. W. S. ANDREWS remarked that the Society was very much indebted to the parent Institution of Civil Engineers for placing their rooms at the disposal of the Society, both for the meetings of council and ordinary general meetings; and he moved —"That the cordial vote of thanks of the Society be presented to the Institution of Civil Engineers for their kindness in continuing to lend their rooms to the Society."

Seconded by Sir CHARLES BRIGHT, who, with reference to a remark in the Annual Report, that in future the meetings of the Society would be held on Thursdays instead of Wednesdays as heretofore, remarked that such step was brought about through the inconvenience frequently felt in getting apparatus (such as the dynamo-electric machines) fitted up, from want of time between the meetings of the Institution of Civil Engineers on Tuesdays and those of the Society on Wednesdays.

Vote of thanks duly accorded.

Mr. E. GRAVES: I think there is another duty incumbent upon the Society that we should not fail to discharge, and which I trust you will fulfil most enthusiastically and fervently. There is a gentleman who has assisted us to a very marked degree and in most important matters, whose labours have hitherto been unacknowledged. I refer to our honorary solicitor, Mr. Bristows. It is not only on general grounds, and for reasons extending over many years, but on account of the careful and assiduous way in which he has worked in the interest of the Society during the past year, that I ask for your present attention. You are aware that by the condition of the trust-deed accompanying the bequest of the Ronalds Library to the Society, it was required that within five years of the presentation application for a royal charter should be

made. You have been informed that that application has been made and declined, but the fact that it was unsuccessful does not make us the less obliged to Mr. Bristows for the trouble he took in connection with the matter. In point of fact, had the Society been a profitable and well-paying client instead of being a very barren cause of trouble, Mr. Bristows could not have done more. I have had various communications with him in reference to the proceedings he conducted. He drafted our petition, made arrangements for presentation, obtained signatures of members of council in its support, and in short, did everything possible. Therefore, I think I may rely upon your giving an enthusiastic and fervent vote of thanks to Mr. Bristows for the general interest he has taken in the Society, and also for the special exertions he has made on its behalf during the past year.

Mr. CROMWELL F. VARLEY: I have the greatest pleasure in seconding the motion of Mr. E. Graves. I think I am right in saying that no member of the Council is in a better position than myself to speak accurately as to the great value of the services rendered to Telegraphy by the firm of Wilson, Bristows & Co.

They worked with Messrs. Fothergill-Cooke & Wheatstone in securing their patents, and in their tedious legal work years before the Pioneer Telegraph Company, viz., the Electric Telegraph Company, was established by royal charter in 1846. During the continuance of that Company up to 1871 they, under the following engineers—W. H. Hatcher, Edwin Clark, J. Latimer Clark, myself, and lastly Mr. Cully, carried out the very varied, complicated, and numerous agreements entered into between the Electric Telegraph Company and the various Railway Companies and other bodies.

Their experience was great, indeed, in electric matters, and by acting as our honorary solicitor, Mr. Bristows proves how deeply he has identified himself with the great object our Society was formed to encourage and foster.

Telegraphy is no longer a merely national institution, it is international, and so long as Electric Telegraphs exist will our honorary solicitor's name occupy a prominent place.

I have the greatest pleasure, while seconding the motion, of being able to bear testimony to Mr. Bristows' deserts.

Mr. BRISTOWE thanked the members, and said that he always had pleasure in doing what he could for the Society, which had never caused him a great amount of trouble, and would be happy to offer his services as he had hitherto done.

The Annual General Meeting was then declared adjourned, and the meeting resolved itself into the General Meeting of Members, for the purpose of considering the proposed alteration to the Title of the Society and alterations to rules.

The PRESIDENT: The first proposition is as decided by the Council—"That the Title of the Society be altered by the addition after the word 'Engineers' of the words 'and of Electricians;' and that the Council be empowered to take all necessary steps for giving full effect to such alteration."

Mr. ALEXANDER SIEMENS seconded the proposition, and said, that since the Society of Telegraph Engineers was first started, the varied applications of electricity had grown to such importance, and the workers in them differed so much from Telegraph Engineers, that it seemed desirable to adopt for the Society a more general Title. The best Title would obviously be "Society of Electricians" or a similar one, but as the Telegraph Engineers had formed the nucleus from which the Society has originated, their name should be allowed to remain in the Title, which in its new form would be sufficiently comprehensive to include all future additions.

Mr. A. LE NEVE FOSTER objected to any addition to the already existing lengthy title. No doubt the Committee had fully considered the subject; but in his opinion, notwithstanding the reasons given for the proposed alteration, he was of opinion that the tacking-on process was both undesirable and unnecessary.

Mr. H. C. DONOVAN maintained that as the principal objects of the Society were purely electrical the proposed alteration should be a prefix and not an affix, such as in other societies—the "Chemical," the "Geological," "Mechanical Engineers," "Physical," &c.

Mr. CROMWELL F. VARLEY: I would just mention that as regards Mr. Donovan's proposal a section of the Chemical Society has lately had under discussion the question of forming a "Society

of Chemical Engineers" It has been felt by many that chemistry is too large a subject for one Society, and, further, the appliances necessary for chemical manufacture, involved a great deal that is in reality engineering.

Mr. E. ALMACK thought the additional "of" was unnecessary.

The PRESIDENT: That little preposition was the subject of much discussion, and it was ultimately decided by the Council, with the approval of the Hon. Solicitor, that to make the title properly comprehensive the word "of" should also be added.

Professor W. E. AYRTON asked if it would not be well if the members were given some idea of what the Council had done in the matter. The point to be considered was not what would have been the best title for the Society years ago, but rather what was best to do now so as to imply that the Society included (and was most anxious to include) among its members those who were in no sense engineers. The late Professor Clerk Maxwell could not be called an engineer in any sense, Dr. Tyndall, our new president Professor Foster, as well as Professor Adams, probably would not classify themselves as telegraph engineers, and yet these were among the most valuable members of the Society. No doubt, if the Society were only now being started the proper title would be "The Electrical Society," but that was not the question. What was before them to-night was a modification of a title that already existed. All sorts of suggestions had been brought forward by the Council and discussed, and the resolution come to was embodied in the proposition of the Council before the meeting. The new title was but a makeshift, and they had to determine on the best makeshift.

The President formally put the proposition to the meeting, and it was declared to be carried.

The PRESIDENT: The next proposal is that rule 7 be amended by striking out the words "and the Secretary," and substituting "Secretary and Librarian." The rule is:—

"7. The officers of the Society shall be a *President*, four *Vice-*
" *Presidents*, and twelve other *Members*, who shall constitute the
" *Council* to direct and manage the affairs of the Society; also

" two Auditors of accounts, two or more Trustees, a Treasurer, an " Honorary Secretary, an Honorary Solicitor, and a Secretary. " Such officers shall be elected or appointed in manner hereinafter " directed. In addition to the above, three *Associates* shall be " elected annually to serve on the Council. The Past Presidents " shall be Honorary Members of Council."

Looking into this rule it was found that by its wording the Honorary Secretary and Honorary Treasurer were not practically Members of Council, and that now a Librarian has been appointed provision should be made for him in this rule. Therefore it is proposed to add the words "and a Librarian" to the list of officers, and that to the list of officers forming the Council shall be added the words "the Honorary Secretary and the Honorary " Treasurer for the time being shall be members of the Council " *ex officio*." Practically they already act as such, but by the improper construction of the rule they were not really members. Your vote is therefore asked to make these two most useful and necessary additions to the rule.

Having been duly put and seconded the propositions were approved.

The next proposition is that to the 8th rule shall be added the words "the Librarian," so that the rule shall read :—" 8. All the " officers except Solicitor, Trustees, Auditors, Secretary, and the " Librarian, shall be elected from the class of Members."

Carried.

To the 9th Rule it is proposed to add the words—" and the Librarian," so that the rule shall then read :—" 9. All offices shall " be honorary except that of Secretary and Librarian."

Carried.

Mr. F. C. Webb asked whether the desirability of a general index to the proceedings of the Society from its commencement had been discussed at all. When making references he had been much delayed by having to make a separate reference for each part, when a general index could either be brought up to date, or at least made to cover a given number of volumes. The Civil Engineers had an index of the kind to every 20 volumes of their proceedings.

Professor AYRTON (as Chairman of the Editing Committee) said that he was pleased to be able to mention that the subject had been very carefully considered, and that a kind offer made by the Librarian (Mr. Frost) to compile such an index for the Society up to the end of the year, or perhaps up to the end of the next volume, No. X., had, he need hardly say, been accepted.

The scrutineers then presented the result of the ballot, when it was declared that the officers elected for the ensuing year were as follows:—

*President:*
Professor G. C. FOSTER, F.R.S.

*Vice Presidents:*

Lieut.-Col. WEBBER, R.E.
WILLOUGHBY SMITH.
Professor W. G. ADAMS, F.R.S.
C. E. SPAGNOLETTI, M.I.C.E.

*Honorary Treasurer:*
EDWARD GRAVES.

*Honorary Secretary:*
Colonel FRANK BOLTON.

*Members of Council:*

W. S. ANDREWS.
W. T. ANSELL.
SIR CHARLES T. BRIGHT, M.I.C.E.
H. G. ERICHSEN.
Col. GLOVER, R.E.
Professor D. E. HUGHES, F.R.S.
A. STROH.
C. F. VARLEY, F.R.S.
H. C. FORDE, M.I.C.E.
E. B. BRIGHT.
ALEXANDER SIEMENS.

*Associate Members of Council:*

Lieut. P. CARDEW, R.E.
J. F. MOULTON, F.R.S.
R. E. CROMPTON.

A vote of thanks was passed to the scrutineers.

The PRESIDENT then declared the meeting adjourned.

# ORIGINAL COMMUNICATIONS.

[The following has been communicated by the author to the Society, as the *authentic* account of his recent experiments.]

## UPON THE PRODUCTION AND REPRODUCTION OF SOUND BY LIGHT.

BY ALEXANDER GRAHAM BELL, PH.D.

*(A Paper read before the American Association for the Advancement of Science, in Boston, on the 27th August, 1880.)*

In bringing before you some discoveries made by Mr. Sumner Tainter and myself, which have resulted in the construction of apparatus for the production and reproduction of sound by means of light, it is necessary to explain the state of knowledge which formed the starting point of our experiments.

I shall first describe that remarkable substance "Selenium," and the manipulations devised by previous experimenters; but the final result of our researches has widened the class of substances sensitive to light-vibrations, until we can propound the fact of such sensitiveness being a general property of all matter.

We have found this property in gold, silver, platinum, iron, steel, brass, copper, zinc, lead, antimony, German silver, Jenkins' metal, Babbitt's metal, ivory, celluloid, gutta percha, hard rubber, soft vulcanised rubber, paper, parchment, wood, mica, and silvered glass; and the only substances from which we have not obtained results are carbon and thin microscope glass. Later experiments have shown that these are not exceptional.

We find that, when a vibratory beam of light falls upon these substances, *they emit sounds*, the pitch of which depends upon the frequency of the vibratory change in the light.

We find, further, that when we control the form or character of

the light-vibrations on selenium (and probably on the other substances), we control the quality of the sound, and obtain all varieties of articulate speech. We can thus, without a conducting wire as in electric telephony, speak from station to station wherever we can project a beam of light. We have not had the opportunity of testing the limit to which this photophonic effect may be extended, but we have spoken to and from points 213 metres apart; and there seems no reason to doubt that the results will be obtained at whatever distance a beam of light can be flashed from one observatory to another. The necessary privacy of our experiments hitherto has alone prevented any attempts at determining the extreme distance at which this new method of vocal communication will be available.

I shall now speak of Selenium.

### *Selenium.*

In the year 1817, Berzelius and Gottlieb Gahn made an examination of the method of preparing sulphuric acid in use at Gripsholm, and during the course of their examination they observed in the acid a sediment of a partly reddish, partly clear brown colour, which, under the action of the blow-pipe, gave out a peculiar odour, like that attributed by Klaproth to tellurium. As tellurium was a substance of extreme rarity, Berzelius attempted its production from this deposit; but he was unable, after many experiments, to obtain further indications of its presence. He found plentiful signs of sulphur mixed with mercury, copper, tin, zinc, iron, arsenic, and lead, but no trace of tellurium.

It was not in the nature of Berzelius to be disheartened by this result. In science, every failure advances the boundary of knowledge as well as every success, and Berzelius felt that, if the characteristic odour that had been observed did not proceed from tellurium, it might possibly indicate the presence of some substance then unknown to the chemist. Urged on by this hope he returned with renewed ardour to his work.

He collected a great quantity of the material, and submitted the whole mass to various chemical processes. He succeeded in separating successively the sulphur, the mercury, the copper, the

tin, and the other known substances whose presence had been indicated by his tests, and, after all these had been eliminated, there still remained a residue, which proved upon examination to be what he had been in search of—*a new elementary substance.*

The chemical properties of this new element were found to resemble those of tellurium in such a remarkable degree that Berzelius gave to the substance the name of "Selenium," from the Greek word σελήνη, the moon ("Tellurium," as is well known, being derived from *tellus*, the earth).

Although tellurium and selenium are alike in many respects, they differ in their electrical properties, tellurium being a good conductor of electricity, and selenium, as Berzelius showed, a non-conductor.

Knox* discovered in 1837 that selenium became a conductor when fused; and Hittorff,† in 1851, showed that it conducted at ordinary temperatures when in one of its allotropic forms.

When selenium is rapidly cooled from a fused condition, it is a non-conductor. In this, its "vitreous" form, it is of a dark brown colour, almost black by reflected light, having an exceedingly brilliant surface. In thin films it is transparent, and appears of a beautiful ruby red by transmitted light.

When selenium is cooled from a fused condition *with extreme slowness*, it presents an entirely different appearance, being of a dull lead colour, and having throughout a granular or crystalline structure, and looking like a metal. In this form it is opaque to light, even in very thin films. This variety of selenium has long been known as "granular" or "crystalline" selenium, or, as Regnault called it, "metallic" selenium. It was selenium of this kind that Hittorff found to be a conductor of electricity at ordinary temperatures. He also found that its resistance to the passage of an electrical current diminished continuously by heating up to the point of fusion, and that the resistance suddenly increased in passing from the solid to the liquid condition.‡

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* *Trans. Roy. Irish Acad.*, 1839, vol. xix., p. 147; also *Phil. Mag.*, (3rd ser.), vol. xvi., p. 185.

† *Pogg. Anna.*, vol. lxxxiv., p. 214; also *Phil. Mag.*, (4th ser.), vol. iii., p. 546.

‡ See Draper and Moss in *Proc. Roy. Irish Acad.*, November, 1873, (2nd ser.), vol. i., p. 529.

It was early discovered that exposure to sunlight* hastens the change of selenium from one allotropic form to another, and this observation is significant in the light of recent discoveries.

Although selenium has been known for the last sixty years, it has not yet been utilised to any extent in the arts, and it is still considered simply as a chemical curiosity. It is usually supplied in the form of cylindrical bars. These bars are sometimes found to be in the metallic condition, but more usually they are in the vitreous, or non-conducting form.

It occurred to Willoughby Smith that, on account of the high resistance of crystalline selenium, it might be usefully employed at the shore end of a submarine cable in his system of testing and signalling during the process of submersion.

Upon experiment, the selenium was found to have all the resistance required, some of the bars employed measuring as much as 1,400 megohms, a resistance equivalent to that which would be offered by a telegraph wire long enough to reach from the earth to the sun.

But the resistance was found to be extremely variable. Efforts were made to ascertain the cause of this variability, and it was discovered that *the resistance was less when the selenium was exposed to light than when it was in the dark.*

This observation, which was first made by Mr. May† (Mr. Willoughby Smith's assistant stationed at Valentia), was soon verified by a careful series of experiments, the results of which were communicated by Mr. Willoughby Smith‡ to the Society of Telegraph Engineers on the 12th of February, 1873.

Platinum wires were inserted into each end of a bar of crystalline selenium, which was then hermetically sealed in a glass tube, through the ends of which the platinum wires projected for the purpose of connection. One of these bars was placed in a box,

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* *Gmelins' Handbook of Chemistry*, 1849, vol. ii., p. 235. See also Hittorff in the *Phil. Mag.* for 1852, (4th ser.), vol. iii., p. 547.

† See lecture by Dr. C. W. Siemens in *Proc. Roy. Inst. of Great Britain*, vol. viii., p. 68.

‡ *Journ. of Soc. of Teleg. Engin.*, vol. ii., p. 73; *Nature*, vol. vii., p. 302; *Amer. Journ. of Science and Arts*, 1873, vol. cv., (3rd ser.), p. 301.

the lid of which was closed so as to shade the selenium, and th resistance of the substance was measured. Upon opening the l of the box the resistance instantaneously diminished.

When the light of an ordinary gas burner (which was plac at a distance of several feet from the bar) was intercepted shading the selenium with the hand, the resistance increased; a upon passing the light through rock salt, and through glasses various colours, the resistance was found to vary according to t amount of light transmitted.

In order to be certain that temperature had nothing to do wi the effect, the selenium was placed in a vessel of water, so that t light had to pass through a considerable depth of water in order reach the selenium. The effects, however, were the same as befo

When a strong light from the ignition of a narrow band magnesium was held about nine inches above the water, the resi ance of the selenium immediately fell more than two-thir returning to the normal condition upon the removal of the light.

The announcement of these results naturally created an inte interest among scientific men; and letters of inquiry regarding t details of the experiments soon appeared in the columns of *Natu* from Harry Napier Draper* and M. L. Sale,† which were answer in the next number by Willoughby Smith.‡ Sale and Drap were soon able to corroborate the statements that had been ma by Willoughby Smith. Sale§ presented his researches to t Royal Society on the 8th of May, 1873, and, in the followi November, Draper‖ presented his results to the Royal Iri Academy, in the shape of a joint paper by himself and Richa J. Moss.

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* *Nature*, vol. vii., p. 340, March 6th, 1873. † *Ibid.*

‡ *Nature*, vol. vii., p. 361, March 13th, 1873.

§ *Proc. Roy. Soc.*, vol. xxi., p. 283. See also *Pogg. Ann.*, CL., p. 333; *Phil. M* 4th ser.), xlvii., p. 216; *Nature*, vol. viii., p. 134.

‖ *Proc. Roy. Irish Acad.*, (2nd ser.), Nov. 10th, 1873, vol. i., p. 529. See also communication from Richard J. Moss to *Nature*, August 12th, 1875, vol. xi p. 291, being an answer to a letter from J. E. H. Gordon upon the "Anomalo Behaviour of Selenium," published in that journal on the 8th of July, 1875 (s vol. xii., p. 187).

Draper and Moss gave in their paper an admirable summary of he condition of our knowledge regarding selenium at that time. They confirmed Hittorff's observation that the temperature of minimum resistance of granular selenium was somewhere about 10° C., and that at 217° C. (the fusing point) the resistance ddenly increased. They carried the temperature to a still higher oint than Hittorff had done, and found that the resistance again minished, reaching a second minimum at 250° C.

During the course of their experiments they produced a variety f granular selenium, not different in appearance from other pecimens, but having different electrical properties. In this form he resistance became greater, instead of less, when the temperature as raised. They also used thin plates of selenium, instead of the ylindrical bars formerly employed, and found great advantage om the increased sensitiveness of these plates to light.

Sale found, upon exposing selenium to the action of the solar pectrum, that the maximum effect was produced just at or outside he extreme edge of the red end of the spectrum, in a point nearly oincident with the maximum of the heat rays, thus rendering it ncertain whether the effect was due to light or to radiant heat.

In the winter of 1873 the Earl of Rosse* attempted to decide his question, by comparing the selenium effects with the indications of the thermopile. He exposed selenium to the action of on-luminous radiations from hot bodies, but could produce no ffect, whereas a thermopile under similar conditions gave abundant indications of a current. He also cut off the heat rays of low efrangibility from luminous bodies by the interposition of glass nd alum between the selenium and the source of light, without naterially affecting the result; but, when the thermopile was mployed, the greater portion of the heat effect was cut off.

Later, Professor W. G. Adams,† of King's College, took up the

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* *Phil. Mag.* (4th ser.), March, 1874, vol. xlvii., p. 161. See also *Amer. Jour. of Science and Arts* (3rd ser.), vol. vii., p. 512.

† *Proc. Roy Soc.*, June 17th, 1875, vol. xxiii., p. 535. See also *Proc. Roy. Soc.*, Nov. 10th, 1875, vol. xxiv., p. 163; *Nature*, Jan. 20th, 1876, vol. xiii., p. 238; *Nature*, March 23rd, 1876, vol. xiii., p. 419; *Scient. Amer. Supplement*, June 3rd, 1876, vol. i., p. 354.

question, and his experiments seemed to prove conclusively that the action was due principally, if not entirely, to those rays of the spectrum which were visible.

This conclusion was supported by the marked effect produced by the light of the moon, and by the apparent insensitiveness of selenium to rays passed through a solution of iodine in bisulphide of carbon. He found that the maximum effect was produced by the greenish yellow rays, and that *the intensity of the action depended upon the illuminating power of the light, being directly as the square root of that illuminating power.*

Professor Adams and Mr. B. E. Day* continued these researches, and, among other interesting and suggestive results, discovered that light produces in selenium an electro-motive force without the aid of a battery.

The most sensitive variety of selenium that has yet been produced was obtained in Germany by Dr. Werner Siemens, by continued heating for some hours at a temperature of 210° C., followed by extremely slow cooling.

Dr. W. C. Siemens,† in a lecture delivered before the Royal Institution of Great Britain, on the 18th of February, 1876, stated that his brother's modification of selenium was so sensitive to light that its conductivity was *fifteen times as great in sunlight as it was in the dark.*

In Werner Siemens'‡ experiments, special arrangements were made for reducing the resistance of the selenium.

For this purpose two fine platinum wires were coiled into a double flat spiral, and were laid upon a plate of mica, so that they did not come into contact with one another. A drop of melted selenium was then placed upon the platinum wire arrangement,

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* *Proc. Roy. Soc.*, May 18th, 1876, vol. xxv., p. 113.

† *Proc. Roy. Inst. Gt. Britain*, Feb. 18th, 1876, vol. viii., p. 68. See also *Nature*, vol. xiii., p. 407; *Scient. Amer. Supplement*, April 1st, 1876, vol. i., p. 222; *Scient. Amer. Supplement*, June 10th, 1876, vol. i., p. 375.

‡ *Monatsbericht der Kön. Preuss. Akad. der Wissenschaften zu Berlin for* 1875, p. 280; *Phil. Mag.*, Nov. 1875 (4th ser.), vol. l., p. 416; *Nature*, Dec. 5th, 1875, vol. xiii., p. 112; *Monatsber. Berl. Akad.*, Feb. 17th, 1876; *Pogg. Ann.*, CL. ix., p. 117; *Monatsber. Berl. Akad.*, June 7th, 1877; *Pogg. Ann.*, 1877, ii., p. 521.

and a second sheet of mica was pressed upon the selenium, so as to cause it to spread out and fill the spaces between the wires.

Each cell was about the size of a silver dime.* The cells were then placed in a paraffin bath and annealed. Siemens devised other arrangements for reducing the resistance. In the form known as "Siemens' grating," the two wires, instead of being coiled together, were arranged in a zig-zag shape, forming a sort of platinum gridiron. This was treated in the same way as the spiral arrangement.

Another form of cell consisted of a sort of lattice-work or basket-work of platinum wires arranged upon a perforated mica plate, the wires interlaced with one another and with the mica plate, so as to make metallic contact only with alternate wires.

He also found that iron and copper might be employed instead of platinum.

Without dwelling further upon the researches of others, I may say that all observations concerning the effect of light upon the conductivity of selenium had been made by means of the galvanometer; but it occurred to me that the telephone, from its extreme sensitiveness to electrical influences, might be substituted with advantage.

Upon consideration of the subject, however, I saw that the experiment could not be conducted in the ordinary way, for the following reason:—The law of audibility of the telephone is precisely analogous to the law of electric induction. No effect is produced during the passage of a continuous and steady current: it is only at the moment of change from a stronger to a weaker state, or *vice versa*, that any sensible effect is produced, and the amount of effect is exactly proportional to the amount of *variation* in the current.

It was therefore evident that the telephone could only respond to the effect produced in selenium at the moment of change from light towards darkness, or *vice versa*, and that it would be advisable to intermit the light with great rapidity, so as to produce a succession of changes in the conductivity of the selenium, correspond-

* The *dime* is a small silver coin about the size of a fourpenny bit. Its value is 10 cents.

ing in frequency to musical vibrations within the limits of the sense of hearing; for I had often noticed that currents of electricity, so feeble as hardly to produce any audible effects from a telephone when the circuit was simply opened or closed, caused very perceptible musical sounds when the circuit was rapidly interrupted, and that the higher the pitch of the sound the more audible was the effect. I was much struck by the idea of producing sound in this way by the action of light. I proposed to pass a bright light through one of the orifices in a perforated screen, consisting of a circular disc or wheel, with holes near the circumference.

Upon rapidly rotating the disc, an intermittent beam of light would fall upon the selenium, and a musical tone should be produced from the telephone, the pitch of which would depend upon the rapidity of the rotation of the disc.

Upon further consideration, it appeared to me that all the audible effects obtained from variations of electricity could also be produced by corresponding variations of light acting upon selenium. I saw that the effect could not only be produced at the extreme distance at which selenium would normally respond to the action of a luminous body, but that this distance could be indefinitely increased by the use of a parallel beam of light, so that we might telephone from one place to another, without the necessity of a conducting wire between the transmitter and the receiver.

It was evidently necessary, in order to reduce this idea to practice, to devise an apparatus to be operated by the voice of a speaker, by which variations could be produced in a parallel beam of light, corresponding to the variations in the air produced by the voice.

I proposed to pass light through a perforated plate containing an immense number of small orifices. Two similarly perforated plates were to be employed: one was to be fixed, and the other attached to the centre of a diaphragm actuated by the voice, so that the vibration of the diaphragm would cause the movable plate to slide to and fro over the surface of the fixed plate, thus alternately enlarging and contracting the free orifices for the passage of light. In this way the voice of a speaker would control the

amount of light passed through the perforated plates, without completely obstructing its passage. This apparatus was to be placed in the path of a parallel beam of light, and the undulatory beam emerging from the apparatus could be received at some distant place upon a lens or other apparatus, by means of which it could be condensed upon a sensitive piece of selenium placed in a local circuit with a telephone and galvanic battery.

The variations in the light produced by the voice of the speaker should cause corresponding variations in the electrical resistance of the selenium at the distant place, and the telephone in circuit with the selenium should reproduce audibly the articulation of th speaker. I obtained some selenium for the purpose of making the experiment described, but found that its resistance was almost infinitely greater than that of any telephone that had been constructed; and I was unable at that time to obtain audible effects in the way desired. I believed, however, that the obstacle could be overcome by devising mechanical arrangements for reducing the resistance of the selenium, and by constructing special telephones for the purpose.

I felt so much confidence in this, that, in a lecture delivered before the Royal Institution of Great Britain, upon the 17th of May, 1878, I announced the possibility of hearing a shadow, by interrupting the action of light upon selenium. A few days afterwards my ideas upon this subject received a fresh impetus by the announcement made by Mr. Willoughby Smith* before the Society of Telegraph Engineers, that he had heard the action of a ray of light falling upon a bar of crystalline selenium, by listening to a telephone in circuit with it. It is not unlikely that the publicity given to the speaking telephone during the last few years may have suggested to many minds, in different parts of the world, somewhat similar ideas to my own: indeed, it has recently come to my knowledge that a writer (J. F. W., of Kew),† on the 13th of June, 1878, asked the readers of *Nature* whether any experiments had been made with the telephone in circuit with a selenium

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* See *Jour. Soc. Tel. Eng.*, May 22rd, 1878, vol. vii., p. 284.

† *Nature*, vol. xviii., p. 169.

galvanic element, arranged as in Sabine's selenium battery,* and suggested that it was not unlikely that sounds would be produced in the telephone by the action of light of variable intensity upon the selenium element in circuit with it.

In September or October, 1878, Mr. A. C. Brown, of London, submitted to me, confidentially, the details of a most ingenious invention of his, of which we may yet hear more.

This invention, although entirely different from my own, involved the use of selenium in circuit with a battery and telephone, and the production of articulate speech by the action of a variable light.

I am also aware that Mr. W. D. Sargent, of Philadelphia, has had some ideas of a similar nature, the details of which I do not know.

I understood from Mr. Sargent that he proposed submitting selenium to the influence of an oscillating beam of light, which should be sent off and on the selenium by the action of the voice. If this is so, the effect produced would be only of an intermittent character, and a musical tone, not speech, would be heard from the telephone in circuit with the selenium.

Although the idea of producing and reproducing sound by the action of light (as described above) was an entirely original and independent conception of my own, I recognise the fact that the knowledge necessary for its conception has been disseminated throughout the civilised world, and that the idea may therefore have occurred independently to many other minds.

I have stated above the few facts that have come under my own observation bearing upon the subject.

*The fundamental idea on which rests the possibility of producing speech by the action of light, is the conception of what may be termed an "undulatory" beam of light, in contradistinction to a merely intermittent one.*

By an undulatory beam of light, I mean a beam that shines continuously upon the sensitive receiver, but the intensity of which upon that receiver is subject to rapid changes corresponding to the changes in the vibratory movement of a particle of air during the transmission of a sound of definite quality through the atmosphere.

* Nature, vol. xvii., p. 512.

The curve that would graphically represent the changes of light would be similar in *shape*, as well as in other respects, to that representing the movement of the air. I do not know whether this conception had been clearly recognised by J. F. W., of Kew, or by Mr. Sargent, of Philadelphia; but to Mr. A. C. Brown, of London, is undoubtedly due the honour of having distinctly and independently formulated the conception, and having devised apparatus, though of a crude nature, for carrying it into execution.

It is greatly due to the genius and perseverance of my friend Mr. Sumner Tainter, of Watertown, Mass., that the problem of producing and reproducing sound by the agency of light has at last been successfully solved.

For many months we have been devoting ourselves to the solution of this problem, and I have great pleasure in presenting to you to-night the results of our labours.

*Researches of Sumner Tainter and A. Graham Bell.*

The first point to which we devoted our attention was the reduction of the resistance of crystalline selenium within manageable limits.

The resistance of selenium cells employed by former experimenters was measured in millions of ohms; and we do not know of any record of a selenium cell measuring less than 250,000 ohms in the dark.

We have succeeded in producing sensitive selenium cells measuring only 300 ohms in the dark, and 150 ohms in the light.

All former experimenters seemed to have used platinum for the conducting part of their selenium cells, excepting Werner Siemens, who found that iron and copper might be employed. We have discovered that brass, although chemically acted upon by selenium, forms an excellent and convenient material. Indeed, we are inclined to believe that the chemical action between the brass and selenium has contributed to the low resistance of our cells, by forming an intimate bond of union between the selenium and brass.

Melted selenium behaves to other substances as water does to a greasy surface; and we are inclined to think that, when selenium is used in connection with metals not chemically acted upon by it, the points of contact between the selenium and the metal offer

a considerable amount of resistance to the passage of a galvanic current, and thus serve to increase the apparent resistance of the selenium.

By using brass, we have been enabled to construct an immense number of selenium cells of different forms. Time will only admit of my showing you two typical forms, one for use with a lens, and the other with a concave reflector. This cell consists of two brass plates insulated from one another by a sheet of mica. The upper plate has numerous perforations and brass pins attached to the lower plate, pass through these orifices, so that their ends, without touching the upper plate, are flush with its surface.

The annular spaces between the pins and the plate are filled with selenium. The above arrangement forms part of a galvanic circuit, and it will be observed that the current can only pass from the plate to the pin through the selenium.

It will also be seen that, owing to the conical shape of the perforations, the points of closest approximation between the pins and the plate are on the upper surface. As the effect produced by light upon selenium is chiefly a surface action, this arrangement is found to be of great advantage. The second typical cell, instead of having a plane surface upon which light can be condensed by means of a lens, is cylindrical in form, so that it can be placed in the focus of a parabolic reflector. This cell is composed of a large number of brass discs, separated by discs of mica slightly smaller in diameter.

The spaces between the brass discs over the mica are filled with selenium, and the alternate brass discs are metallically connected.

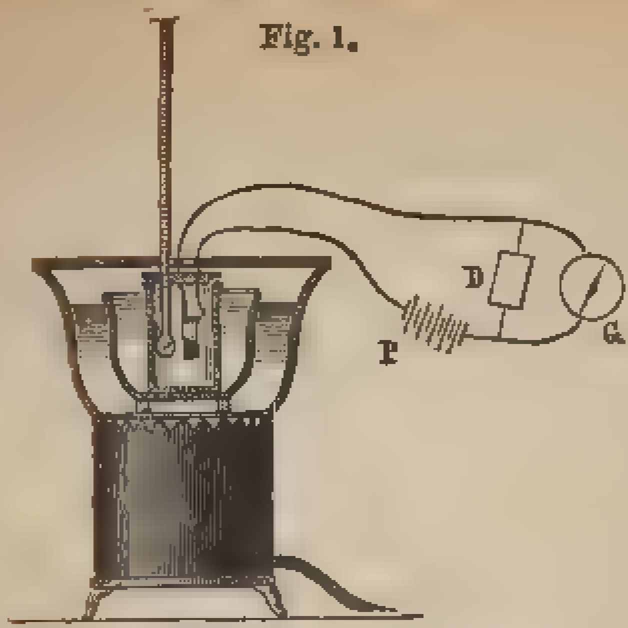
The arrangement practically consists of a large number of annular selenium cells united in multiple arc.

The mode of applying the selenium is as follows:—The cell is heated, and, when hot enough to melt selenium, a stick of that substance is rubbed over the surface. In order to acquire conductivity and sensitiveness, the selenium must next undergo a process of annealing.

The method we first adopted was the following:—

The selenium cell was placed with a thermometer in the interior of the cylindrical annealing chamber shown in Fig. 1.

Fig. 1.




This was inserted in a pot of linseed oil, and the latter stood upon glass supports within another similar pot containing linseed oil. The whole arrangement was placed over a gas stove and heated to a temperature of about 214° C., which was found to be the temperature of maximum conductivity for the selenium used. This temperature was retained for 24 hours, and the pots with their contents were then packed up in a box arranged to retard radiation of heat, so that the selenium took from 40 to 60 hours to cool down to the temperature of the air.

A powerful battery current was passed through the selenium during the whole process of heating and cooling, in accordance with our theory that the current exerted a powerful influence in causing a set of the selenium molecules, and in retaining them in position until fixed by crystallisation. A shunted galvanometer was introduced into the circuit for the purpose of observing the changes of conductivity.

We subsequently found this tedious process to be unnecessary, as during the course of our experiments we discovered a method of preparing sensitive selenium in a very few minutes.

We now simply heat the selenium over a gas stove and observe its appearance. When the selenium attains a certain temperature, the beautiful reflecting surface becomes dimmed. A cloudiness extends over it, somewhat like the film of moisture produced by breathing upon a mirror.

This appearance gradually increases, and the whole surface is soon seen to be in the metallic, granular, or crystalline condition.

The cell may then be taken off the stove and cooled in any suitable way. When the heating process is carried too far, the crystalline selenium is seen to melt.

Our best results have been obtained by heating the selenium until it crystallises as stated above, and by continuing the heating until signs of melting appear, when the gas is immediately put out.

The portions that had melted instantly recrystallise, and the selenium is found upon cooling to be a conductor, and to be sensitive to light. The whole operation occupies only a few minutes.

This method has not only the advantage of being expeditious, but it proves that many of the accepted theories on this subject are fallacious.

Early experimenters considered that the selenium must be cooled from a fused condition with extreme slowness. Later authors agree in believing that the retention of a high temperature short of the fusing point, and slow cooling, are essential, and the belief is also prevalent that crystallisation takes place only during the cooling process.

Our new method shows that fusion is unnecessary, that conductivity and sensitiveness can be produced without long heating and slow cooling, and that crystallisation takes place during the heating process.

We have found, on removing the source of heat immediately upon the appearance of the cloudiness above referred to, that distinct and separate crystals can be observed under the microscope, which appear like leaden snow-flakes on a ground of ruby red.

Upon removing the heat when crystallisation is further advanced, we perceive under the microscope masses of these crystals arranged like basaltic columns, standing detached from one another, and at a still higher temperature the distinct columns are no longer traceable, and the whole mass resembles metallic pudding-stone, with here and there a separate crystal like a fossil on the surface.

Selenium crystals formed during slow cooling after fusion present an entirely different appearance, showing distinct facets.

I must now endeavour to explain the means by which a beam of light can be controlled by the voice.

## *Photophonic Transmitters.*

We have devised upwards of fifty forms of apparatus for varying a beam of light in the manner required, but only a few typical varieties need be described.

The source of light may be controlled, or a steady beam may be modified at some point in its path.

In illustration of the first method, we have devised several forms of apparatus, founded upon Koenig's manometric capsule, operating to cause variations in the pressure of gas supplied to a burner, so that the light can be vibrated by the voice.

In illustration of the second method, I have already described one form of apparatus by which the light is obstructed in a greater or less degree in its passage through perforated plates.

But a beam may be controlled in many other ways. For instance, it may be polarised, and then affected by electrical or magnetical influences in the manner discovered by Faraday and Dr. Kerr.

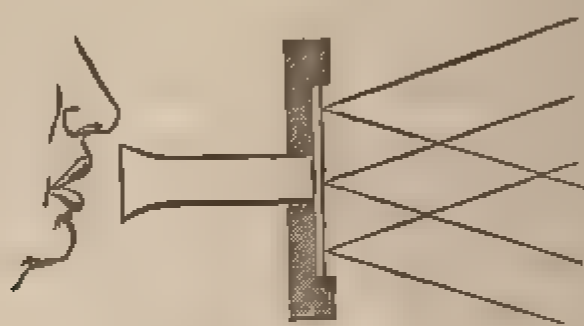
Let a polarised beam of light be passed through a solution of bisulphide of carbon contained in a glass vessel, inside a helix of insulated wire, through which helix is passed an undulatory current of electricity from a microphone or telephonic transmitter operated by the voice of a speaker. The passage of the polarised beam should be normally partially obstructed by a Nicol prism, and the varying rotation of the plane of polarisation would cause more or less of the light to pass through the prism, thus causing an undulatory beam of light capable of producing speech. The beam of polarised light, instead of being passed through a liquid, could be reflected from the polished pole of an electro-magnet in circuit with a telephonic transmitter.

Another method of affecting a beam of light is to pass it through a lens of variable focus,* formed of two sheets of thin glass, containing between them a transparent liquid or gas. The vibrations of the voice are communicated to the gas or liquid, thus causing a vibratory change in the convexity of the glass surfaces,

* I observe that a lens of similar construction has been invented in France by Dr. Cusco, and is described in a recent paper in *La Nature*. See *Scientific American* for August 28, 1880, vol. xlii., p. 131. Mr. Tainter and I have used such a lens in our experiments for months past.

and a corresponding change in the intensity of the light received upon the sensitive selenium. *The simplest form of apparatus for producing the effect consists of a plane mirror of flexible material, such as silvered mica or microscope glass, against the back of which the speaker's voice is directed*, as shown in the diagram, Fig. 2. The light from such a mirror is thrown into vibrations corresponding to those of the diaphragm itself. In its normal condition, a parallel beam of light falling upon the diaphragm mirror would be reflected parallel. Under the action of the voice the mirror becomes alternately convex and concave, and thus alternately scatters and condenses the light. When crystalline selenium is exposed to the undulatory beam reflected from such an apparatus, the telephone connected with the selenium audibly reproduces the articulation of the person speaking to the mirror.

Fig. 2.



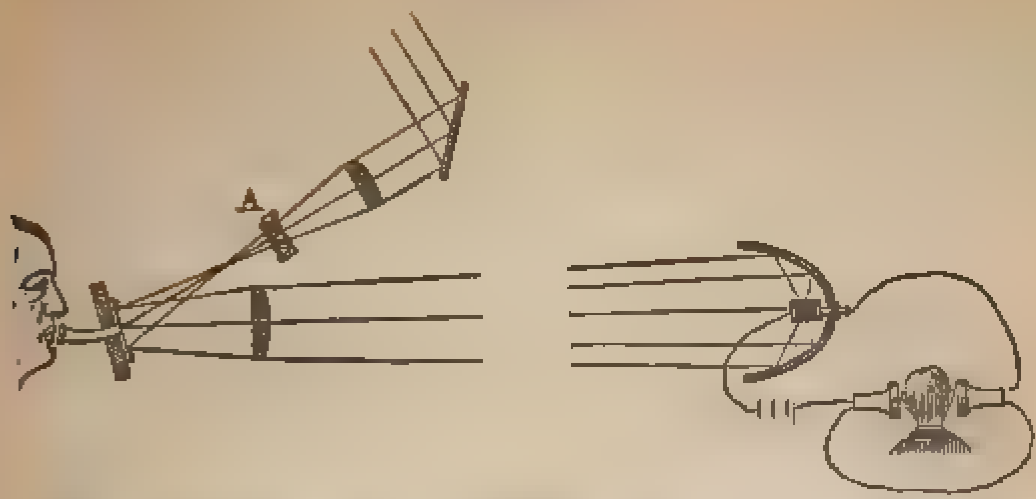
*Arrangement of Apparatus.*

In arranging the apparatus for the purpose of reproducing sound at a distance, any powerful source of light may be used, but we have experimented chiefly with sunlight.

For this purpose a large beam is concentrated by means of a lens upon the diaphragm mirror, and after reflection is again rendered parallel by means of another lens. The beam is received at a distant station upon a parabolic reflector, in the focus of which is placed a sensitive selenium cell, connected in a local circuit with a battery and telephone. The full arrangement is shown in Fig. 3. We have found it advisable to protect the mirror by placing it out of the focal point, and by passing the beam through an alum cell.

A large number of trials of this apparatus have been made

Fig. 3.




with the transmitting and receiving instruments so far apart that sounds could not be heard directly through the air. In illustration, I shall describe one of the most recent of these experiments.

Mr. Tainter operated the transmitting instrument, which was placed on the top of the Franklin Schoolhouse in Washington, and the sensitive receiver was arranged in one of the windows of my laboratory, 1325, L Street, at a distance of 213 metres.

Upon placing the telephone to my ear, I heard distinctly from the illuminated receiver the words, "Mr. Bell, if you hear what I say, come to the window and wave your hat." In laboratory experiments, the transmitting and receiving instruments are necessarily within earshot of one another, and we have therefore been accustomed to prolong the electric circuit connected with the selenium receiver, so as to place the telephones in another room.

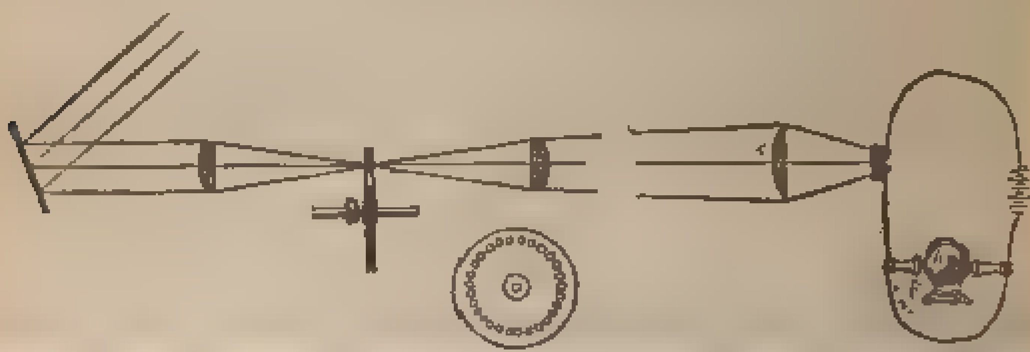
By such experiments we have found that articulate speech can be reproduced by the oxyhydrogen light, and even by the light of a kerosene lamp.

The loudest effects obtained from light are produced by rapidly interrupting the beam, and the most suitable apparatus for doing this seems to be a perforated disc which can be rapidly rotated. The great advantage of this form of apparatus for experimental work is the noiselessness of its operation, admitting of the close approach of the receiver, without interfering with the audibility of the effect heard from the latter; for it will be understood that musical tones are emitted from the receiver when no sound has been made at the transmitter.

A silent motion thus produces a sound. In this way musical tones have been heard even from the light of a candle.

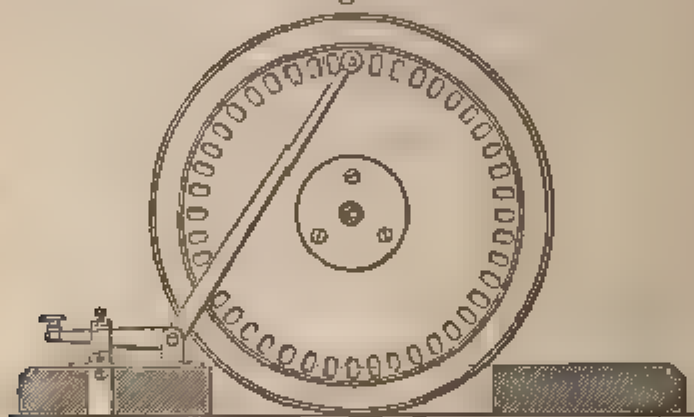
When distant effects are sought, the apparatus is arranged as shown in Fig. 4.

Fig. 4.



By placing an opaque screen near the rotating disc the beam can be entirely cut off by a slight motion of the hand, and musical signals, like the dots and dashes of the Morse telegraph code, can thus be produced at the distant receiving station. Such a screen, operated by a key like a Morse telegraph key, is shown in Fig. 5, and has been operated very successfully.

Fig. 5.



*Experiments to ascertain the Nature of the Rays that affect Selenium.*

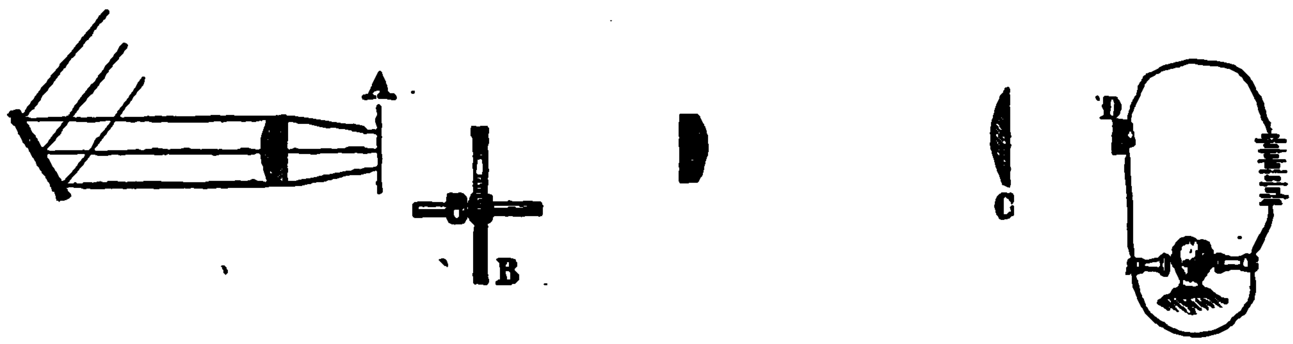
We have made experiments with the object of ascertaining the nature of the rays that affect selenium.

For this purpose we have placed in the path of an intermittent beam various absorbing substances. Professor Cross has been kind enough to give us his assistance in conducting these experiments.

When a solution of alum or bisulphide of carbon is employed, the loudness of the sound produced by the intermittent beam is very slightly diminished; but a solution of iodine in bisulphide of carbon cuts off most but not all of the audible effect. Even an

apparently opaque sheet of hard rubber does not entirely do this. This observation, which was first made in Washington by Mr. Tainter and myself, is so curious and suggestive, that I give in full the arrangement for studying the effect.

Fig. 6.




When a sheet of hard rubber (A) was held between the perforated disc and the source of light, as shown in the diagram (Fig. 6), the rotation of the disc or wheel (B) interrupted what was then an *invisible beam*, which passed over a space of several metres before it reached the lens (C) which finally concentrated it upon the selenium cell (D). A faint but perfectly perceptible musical tone was heard from the telephone connected with the selenium, which could be interrupted at will by placing the hand in the path of the invisible beam.

It would be premature, without further experiments, to speculate too much concerning the nature of these invisible rays; but it is difficult to believe that they can be heat rays, as the effect is produced through two sheets of hard rubber, having between them a glass vessel containing a saturated solution of alum. Although effects are produced as above shown by forms of radiant energy which are invisible, we have named the apparatus for the production and reproduction of sound in this way, "*The Photophone*," because an ordinary beam of light contains the rays which are operative.

### *Non-Electric Photophonic Receivers.*

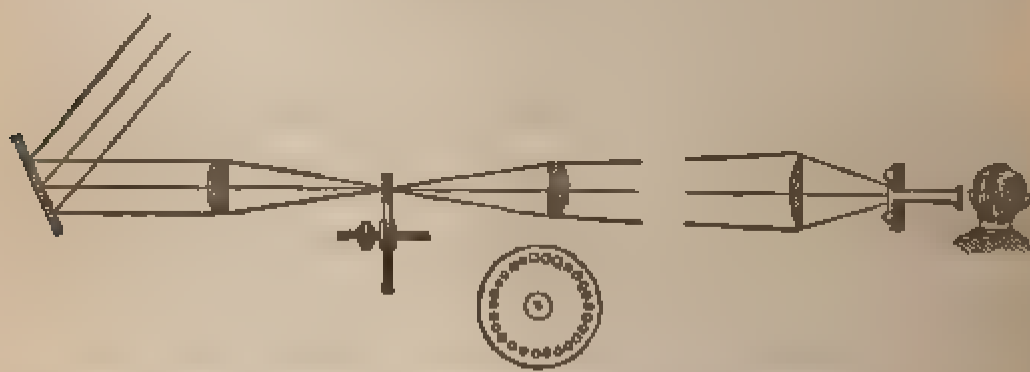
It is a well-known fact that the molecular disturbance produced in a mass of iron by the magnetising influence of an intermittent electrical current can be observed as sound by placing the ear in close contact with the iron; and it occurred to us that the molecular disturbance produced in crystalline selenium by the action of an intermittent beam of light should be audible in a similar manner, without the aid of a telephone or battery.

Many experiments were made to verify this theory, but at first without definite results.

The anomalous behaviour of the hard rubber screen, alluded to above, suggested the thought of listening to it also.

This experiment was tried with extraordinary success. I held the sheet in close contact with my ear, while a beam of intermittent light was focussed upon it by means of a lens. A distinct musical note was immediately heard. We found the effect intensified by arranging the sheet of hard rubber as a diaphragm, and listening through a hearing tube, as shown in Fig. 7.

Fig. 7.



We then tried crystalline selenium in the form of a thin disc, and obtained a similar but less intense effect. The other substances which I enumerated at the commencement of my address were now successively tried in the form of thin discs, and sounds were obtained from all but carbon and thin glass.*

In our experiments, one interesting and suggestive feature was the different intensities of the sounds produced from different substances under similar conditions.

We found hard rubber to produce a louder sound than any other substance we tried, excepting antimony and zinc, and paper and mica to produce the weakest sounds.

On the whole, we feel warranted in announcing, as our conclusion, that *sounds can be produced by the action of a variable light from substances of all kinds, when in the form of thin diaphragms.*

The reason why thin diaphragms of the various materials are more effective than masses of the same substances appears to be that the molecular disturbance produced by light is chiefly a surface

* We have since obtained perfectly distinct tones from carbon and thin glass.

action, and that the vibration has to be transmitted through the mass of the substance in order to reach the ear. On this account we have endeavoured to lead to the ear, air that is directly in contact with the illuminated surface, by throwing the beam of light upon the interior of a tube, and very promising results have already been obtained.

Fig. 8.

Fig. 8 shows the arrangement we have tried. We have heard from interrupted sunlight very perceptible musical tones through tubes of ordinary vulcanised rubber, of brass, and of wood. These were all the materials at hand in tubular form, and we have had no opportunity since of extending the observations to other substances.* I am extremely glad that I have the opportunity of making the first publication of these researches before a scientific society, for it is from scientific men that my work of the last six years has received its earliest and kindest recognition.

I gratefully remember the encouragement which I received from the late Professor Henry, at a time when the speaking telephone existed only in theory. Indeed, it is greatly due to the stimulus of his appreciation that the telephone became an accomplished fact. I cannot state too highly, also, the advantage I received in preliminary experiments on sound vibrations, in this building, from Professor Cross, and near here from my valued friend Dr. Clarence J. Blake.

* We have since tried tubes of hard rubber very successfully, and have even produced the sensation of sound by throwing light into the ear itself. This last experiment was tried a number of times unsuccessfully by myself and friends, until the proper position of the ear was discovered. I have heard a faint but perfectly distinct musical tone from an intermittent beam of sunlight focussed into my ear. The beam was filtered through a solution of alum, but a considerable amount of heat was of course perceptible, and the experiment was only continued for a sufficiently long period of time for me to satisfy myself of the reality of the phenomenon.

When the public were incredulous of the possibility of electrical speech, the American Academy of Arts and Sciences, the Philosophical Society of Washington, and the Essex Institute of Salem recognised the reality of the results, and honoured me by their congratulations.

The public interest, I think, was first awakened by the judgment of the very eminent scientific men before whom the telephone was exhibited in Philadelphia, and by the address of Sir William Thompson before the British Association for the Advancement of Science.

At a later period, when even practical telegraphers considered the telephone as a mere toy, several scientific gentlemen, Professor John Pierce, Professor Eli W. Blake, Dr. Channing, Mr. Clarke, and Mr. Edson S. Jones, of Providence, Rhode Island, devoted themselves to a series of experiments for the purpose of assisting me in making the telephone of practical use, and they communicated to me, from time to time, the results of their experiments, with a kindness and generosity I can never forget.

It is not only pleasant to remember these things, and to speak of them, but it is a duty to repeat them, as they give a practical refutation to the often-repeated stories of the blindness of scientific men to unaccredited novelties, and of their jealousy of unknown inventors who dare to enter the charmed circle of science.

I trust that the scientific favour which was so readily accorded to the telephone may be extended by you to this new claimant—the Photophone.

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P.S.—Three days after the delivery of the above lecture, I received an official notification from the Minister of Public Instruction in France, that, on the recommendation of the Academy of Sciences, the French Government had awarded to me the Volta Prize for the invention of the Telephone. I think it only right that I should express here my high appreciation of the honour that has been conferred upon me.

ALEXANDER GRAHAM BELL.

# ON INDUCTION BETWEEN PARALLEL WIRES.

By OLIVER HEAVISIDE, *Associate.*

1. Electrical induction is of two kinds, electro-static and electro-magnetic. The electrification of one conductor is always accompanied with electrification of others that may be in its neighbourhood, and one way of expressing this is to say that the charge on the first conductor induces an opposite charge on a neighbouring conductor. Also, any change in the amount of electro-magnetic induction passing through a circuit, is accompanied by an E.M.F. in the circuit proportional to the rapidity of the change, which E.M.F. produces a current in the circuit. The change in the amount of induction may be due to a change in the current flowing through another circuit in the neighbourhood, or to relative motion of the circuits, or to changes in the magnetic field due to other causes, as the motion of a magnet for instance. In any case, the transient current accompanying the change is said to be induced. Both inductions are in action together, which considerably complicates the matter, but either one or the other may be frequently ignored for the time, without serious loss of accuracy.

To illustrate the general nature of induction between parallel wires, it is sufficient to consider two wires. Suppose that both wires were originally free from charge and at potential zero, and that we then put a battery on at the beginning of the first wire, whose remote end is to earth, as are both ends of the second wire. After a little time, a steady current is found to be flowing through the first wire, and no current through the other. The value of the steady current is $\frac{E}{R}$, where E is the potential at the beginning of the first wire and R its resistance. But before this steady current is reached, a somewhat complex state of things exists, due to the action of electro-static and electro-magnetic induction. Considering the electro-static alone in the first place, the surface of the first wire forms one coating of a condenser, of which the other coating is the surface of the earth and of the second wire, which is in connection with the earth. Now one pole of the battery

is connected to the first line, and the other pole to earth, and therefore with the second line. The first line receives a charge, say, positive, while the earth and second wire receive an equal negative charge, the amount of these charges depending on the size and position of the two wires. As the first wire is being charged, a positive current flows in from the battery to do it. The negative charge on the earth and second wire may be considered as resulting from a negative current from the battery to earth and the second wire. Or we may say, using old-fashioned language, that the + electricity on the first wire attracts − from the earth to the second wire. Or that the + charge on the first wire induces a − charge on the earth and second wire. Or that the potential of the second wire due to the + charge on the first is +, therefore a + current must flow from the second wire to earth, until its potential is brought to zero, leaving it negatively charged. Or, more accurately, because more comprehensively, we may consider all the elementary circuits, partly conductive and partly inductive, from one pole of the battery to the first wire, and from the latter to earth direct, and also *viâ* the second wire to the other pole of the battery, in every one of which circuits a + current flows, producing electrical polarization of the dielectric, whose residual polarization appears as a + charge on the first wire, and a − charge on the second wire and the earth. But whatever mode of expression be used the result is the same. The final distribution of potential and charge when equilibrium is reached may be easily stated. By Ohm's law, the final potential $v_1$ of the first wire is $\mathrm{E}\left(1-\frac{x}{l}\right)$, $x$ being the distance from the battery to any point, and $l$ the whole length. And the density $\rho_1$ per unit of length of the first wire is $c_1 v_1 = c_1 \mathrm{E}\left(1-\frac{x}{l}\right)$, where $c_1$ is its electro-static capacity per unit of length. The final potential of the second wire is of course zero. The linear density $\rho_2$ of its charge is $c_{12} v_1 = c_{12} \mathrm{E}\left(1-\frac{x}{l}\right)$; $c_{12}$ being the co-efficient of mutual electro-static capacity of the two wires per unit of length. $c_{12}$, it should be remembered, is −, so that $\rho_2$ is −. (The ratio $-c_{12} : c_1$ may be taken roughly at about $\frac{1}{4}$ when the wires are suspended at the shortest usual distance

apart : the exact value may be readily calculated.) Thus the potential and density of the charge on the first wire fall uniformly from their greatest at the beginning to zero at the distant end, while the density of the charge on the second wire rises from its greatest − value at the beginning to zero at the far end, the densities at corresponding points being roughly as 4 : 1 at the most. Thus the second wire has $\frac{1}{4}$ the opposite charge of the first, and the earth the remaining $\frac{3}{4}$.

2. Now removing the battery and putting earth on instead, the wires are discharged. The charge of the first wire flows out at both ends, reducing its potential to zero, twice as much going out at the battery end as at the other. As its potential falls, that of the second wire also falls: from zero it becomes −. For its potential due to its own − charge is −, and this was only neutralized by the equal + potential due to the original charge of the first wire; so as the latter is reduced the former comes into play. During the whole time the first wire is discharging the potential of the second wire is therefore −, causing a + current from earth at both ends, which lasts until enough electricity has entered to cancel its original − charge, and bring it to potential zero again. Or we may say equivalently that its − charge flows out at both ends simultaneously with the discharge of the first wire, and in the same proportion, $\frac{2}{3}$ at the battery end, and $\frac{1}{3}$ at the other.

3. Now insulate the second wire at both ends, and again apply the battery to the first. It becomes charged, and after a little time a steady current E ÷ R flows through it, and its potential is $E\left(1 - \frac{x}{l}\right)$ as before. But as the second wire is now cut off from earth it cannot receive any charge, that is, its total charge must be zero. Likewise its potential must be +, and of the same value all along.

If $v_1$, $v_2$, are the potentials, $\rho_1$, $\rho_2$, the linear densities at distance $x$; $c_1$ $c_2$, the linear electro-static capacities, and $c_{12}$ the linear mutual electro-static capacity, the potentials and densities are connected by the equations

$$\rho_1 = c_1 v_1 + c_{12} v_2 \qquad\qquad \rho_2 = c_2 v_2 + c_{12} v_1$$

Now, since $v_2$ is constant, and the second line has no charge on the whole,

$$c_2 v_2 l + c_{12} \int v_1 \, dx = 0; \text{ where } v_1 = E\left(1 - \frac{x}{l}\right).$$

From these data it follows that

$$v_2 = -\frac{c_{12}}{2c_2} E; \; \rho_1 = c_1 E\left(1 - \frac{x}{l}\right) - \frac{c_{12}^2}{2c_2} E; \; \rho_2 = E c_{12}\left(1 - \frac{x}{l}\right) - \frac{c_{12}}{2} E.$$

Thus the potential of the second wire becomes uniformly about one-eighth of the potential at the battery end of the first, on the former assumption that $-4\, c_{12} = c_2$. The density of the charge of the first wire falls uniformly from $E\left(c_1 - \frac{c_{12}^2}{2\, c_2}\right)$ at the beginning to $-\frac{c_{12}^2}{2\, c_2} E$ at the end, thus dividing the line into a positively and a negatively charged portion, the length of the latter being (on the same assumption) $\frac{1}{33}$ of the whole length. And the density of the charge on the second wire rises uniformly from $\frac{c_{12}}{2} E$ at the beginning to $-\frac{c_{12}}{2} E$ at the far end; thus the second half is positively, the first negatively charged to the same amount. The total charge of the first is $\frac{1}{2} E\, l \left(c_1 - \frac{c_{12}^2}{c_2}\right)$; of the second, zero.

Thus during the establishment of the steady current in the first wire, there is a current in the second in the same direction, a transfer of electricity from the first half of the line to the second, leaving the former negatively charged and the latter positively. Removing the battery and earthing the first wire, the discharge of the first wire will make the potential of the second wire less in the first half than in the second, so that the disappearance of the first wire's charge is accompanied by a $-$ current in the second, restoring it to zero potential again.

4. These examples are perhaps sufficiently elucidative of the part that electro-static induction plays during the establishment of a current in a wire, and of its influence on the final potentials and densities. All disturbing influences have been of course ignored; perfect earth connections have been supposed, also per-

fect conductivity of the earth, perfect insulation, and absence of earth currents and atmospheric electricity.

When the number of parallel wires is not limited to two, the phenomena, though more complex, are essentially of the same nature. The final states of potential and charge assumed by any number of wires with batteries applied to one or more of them may be found from the general equations—

$$\left.\begin{array}{l} \rho_1 = c_1\, v_1 + c_{12}\, v_2 + c_{13}\, v_3 + c_{14}\, v_4 + \ldots \\ \rho_2 = c_{12}\, v_1 + c_2\, v_2 + c_{23}\, v_3 + c_{24}\, v_4 + \ldots \\ \rho_3 = c_{13}\, v_1 + c_{23}\, v_2 + c_3\, v_3 + c_{34}\, v_4 + \ldots \\ \cdot \quad \cdot \quad \cdot \quad \cdot \quad \cdot \quad \cdot \quad \cdot \quad \cdot \quad \cdot \quad \cdot \end{array}\right\} \cdot \ldots \quad (1.)$$

Here $c_1$, $c_2$, $c_3$, ... are the electro-static capacities of wires 1, 2, 3, ... , $c_{12}$ the mutual capacity of 1 and 2, $c_{13}$ that of 1 and 3, and so on, all per unit of length; $v_1\, v_2\, v_3$ ... the potentials, and $\rho_1\ \rho_2$ ... the densities per unit of length. (See Maxwell's *Electricity*, vol. I., ch. iii.) These equations will be referred to later on.

5. Now there is electro-magnetic induction to be considered. For simplicity suppose it to act alone; and as before, take the case of two parallel wires, both earthed, and apply a battery to the first. During the establishment of the current in the first wire (supposed to take place uniformly all along its length), a current in the *opposite* direction is *induced* in the second (also uniform all along), which ceases when the current in the first reaches its steady strength. And on the cessation of the current in the first wire, a current is *induced* in the second in the *same* direction. But this, though sufficient for many, is but a very rudimentary statement of the case.

According to Thomson and Maxwell's theory, the electric current is a kinetic phenomenon, involving matter in motion, and the motion is not confined to the wire alone, but is to be found wherever the magnetic force of the current extends. As matter has to be set in motion when a current is in course of establishment, inertia has to be overcome, the real inertia of moving matter having the negative property of remaining in the state of motion it may have. So that the current cannot be established instantaneously, but rises gradually. And if the current be left to

itself without any impressed E.M.F. to support it, it does not cease instantaneously, but gradually decays in the same manner as it was set up, in virtue of the real momentum of the moving matter. That it decays at all is due to the production of heat by the current, which is inseparable from its existence, *i.e.*, the kinetic energy of the current is degraded into the kinetic energy of heat. Thus when the source of energy is cut off by removing the battery, the momentum of the current begins immediately to fall off, drawing all the while upon its reserve store of energy to maintain it. Now respecting the currents induced in neighbouring conductors. The momentum exists in all parts of the field, and on the removal of the E.M.F. becomes visible in all of them, the energy becoming degraded into heat in all. Granting this, the currents induced must be all in the same direction, viz., as that in the primary wire, and it follows immediately that on setting up a current, the opposite occurs, currents in the opposite direction to that set up being caused in all the wires. In the secondary wires it is evident as such; in the primary it is evident as retarding the rise of the current.

Not knowing the actual mechanism of the current and of the magnetic force, we cannot know what the actual amount of real momentum is, although the amount of energy, the connecting link between all forces, may be calculated. But, in a dynamical system, it is not at all necessary that the mechanism should be known completely. If the state of the system is completely defined by the values of a certain number of variables, the relations between forces, momenta, &c., corresponding to these variables may be calculated on strictly dynamical principles. Thus Maxwell's electro-magnetic momentum of a circuit bears the same relation to the impressed E.M.F. in the circuit that momentum does to force in ordinary dynamics. Ohm's law, however, remains an experimental fact, and is taken as such alone.

In the case of two circuits, the equation of motion are

$$E = R_1 \gamma_1 + \frac{d}{dt}(L_1 \gamma_1 + M \gamma_2)$$

$$0 = R_2 \gamma_2 + \frac{d}{dt}(L_2 \gamma_2 + M \gamma_1).$$

Here $\gamma_1$ and $\gamma_2$ are the currents at any moment in the circuits 1 and 2 of resistances $R_1$ and $R_2$, and E the impressed E.M.F. in circuit 1. $L_1 \gamma_1 + M \gamma_2$ is the electro-magnetic momentum of the first circuit, and $L_2 \gamma_2 + M \gamma_1$ that of the second; $L_1$, $L_2$ and M being constants depending on the form and position of the circuits. In the first circuit, the E.M.F. E is employed partly in maintaining the current $\gamma_1$ against the resistance $R_1$, and partly in increasing the momentum of the first circuit. In the second circuit, where there is no impressed force, the induced E.M.F. is $-\frac{d}{dt}(L_2 \gamma_2 + M \gamma_1)$, that is, the rate of decrease of its electro-magnetic momentum. Further than this it is only necessary to mention here that the setting up of the current $\frac{E}{R_1}$ in circuit 1 is accompanied by an integral current $\frac{M E}{R_1 R_2}$ in the opposite direction in circuit 2, and the decay of the current in 1 by an equal integral flow in 2 in the + direction.

6. We may now compare together the integral currents of electro-static and electro-magnetic induction in circuit 2, circuits 1 and 2 being two parallel suspended wires for definiteness, earthed at their ends.

If $Q_1$ is the integral electro-magnetic current

$$Q_1 = \frac{M E}{R_1 R_2}.$$

where M is the mutual electro-magnetic capacity of the two wires.

If $Q_2$ is the electro-static charge received by the second wire,

$$Q_2 = \frac{c_{12}\, l\, E}{2};$$

therefore

$$\frac{Q_2}{Q_1} = \frac{R^2}{2} \frac{c_{12}\, l}{M}$$

supposing the wires have the same resistance. Thus $\frac{Q_2}{Q_1}$ increases as the square of the length. One-third of this must be taken for the ratio at the distant end, where $Q_2$ and $Q_1$ are in opposite directions.

If $c_{12} = \cdot 003$ microf. per mile, $M = m l$, and $m = 3$ per

centimetre, or 482,802 per mile; $R = k\,l$, and $k = 15$ ohms per mile,

$$\frac{1}{3}\frac{Q_2}{Q_1} = \frac{l^2}{4291}$$

Thus when $l =$ about 65 miles the integral currents at the receiving end are equal. For a shorter length the electro-static is overpowered by the electro-magnetic and the reverse for a greater length. This calculation is quite a rough one, but will do approximately for two suspended wires at the shortest usual distance apart.

7. Having considered the general nature of electro-static and electro-magnetic induction in the simple case of two wires, and the final distribution of electricity on the wires in equilibrium, the next step is to more accurately determine the manner in which the potential and the current behave in the variable state that supervenes between one state of equilibrium or steady flow and another. In the first place, electro-magnetic induction will be ignored, as it greatly complicates the theory, while it is of quite secondary importance on lines not less than a certain short length.

The fundamental equations, whether we consider one line or many, with or without electro-magnetic induction, are

$$\gamma'_1 + \dot{\rho}_1 = 0\,;\; \gamma'_2 + \dot{\rho}_2 = 0\,;\; \gamma'_3 + \dot{\rho}_3 = 0\,;\; \text{etc.} \quad \ldots \quad (2.)$$

where $\gamma$ stands for current, $\rho$ for linear density, and the suffix denotes the wire referred to. In the following $'$ will be generally used to indicate differentiation with respect to $x$ and $\dot{}$ differentiation with respect to $t$ the time. Thus any one of (2) written fully is

$$\frac{d\gamma}{dx} + \frac{d\rho}{dt} = 0.$$

This is nothing more than the equation of continuity, and it may be proved thus. If $\gamma$ is the current at $x$ at time $t$ then $\gamma + \gamma'\,dx$ is the current at $x + dx$ at time $t$, and the excess of the current at $x$ over that at $x + dx$ is $-\gamma'\,dx$. Therefore in the time $dt$ the excess of the quantity of electricity that has passed $x$ over what has passed $x + dx$ is $-\gamma'\,dx\,dt$, and this quantity must have been added to the charge of $dx$. At time $t$ the latter is $\rho\,dx$ and at time $t + dt$ it becomes $\rho\,dx + \dot{\rho}\,dx\,dt$; whence another

expression for the increase in time $dt$ of the elementary charge of $dx$ is $\dot{\rho}\, dx\, dt$, and the above equation follows.

If the wire exists alone we have $\rho = cv$. Also by Ohm's law, $\gamma = -\frac{1}{k}v'$, where $k$ is the resistance per unit of length. Whence

$$v'' = ck\dot{v} \quad \ldots \quad \ldots \quad \ldots \quad \ldots \quad (3.)$$

with the same equation for $\rho$ or $\gamma$. (It is practically best to work with $v$) This is Sir Wm. Thomson's well-known equation of the potential in a submarine cable, or of course in any uniform wire unaffected by the induction of others. The same equation was given by Ohm for the "tension" of a uniform wire, but singularly enough it was arrived at by an entirely erroneous assumption, viz., that a wire had a capacity or power of absorbing electricity into its substance, just as a conductor of heat has a "capacity" for heat. In fact, he applied to electricity Fourier's equations for the diffusion of heat by conduction. Mathematically considered, it amounts to exactly the same thing, for the purpose of calculating the propagation of signals, whether the electricity is detained in the substance of a wire, or forms a superficial charge, connected by tubes of induction with an equal opposite charge on other conductors separated from it by a dielectric, the so-called surface charge being merely the residual polarization of the dielectric, within which the potential energy of electrification is stored; yet the difference between Ohm's capacity and the real thing is very striking.

The general solution of (3) adapted for suiting limiting conditions is

$$v = \Sigma \text{A sin.} \left(\frac{ax}{l} + b\right) e^{-\frac{a^2 t}{ckl^2}} \quad \ldots \quad \ldots \quad (4.)$$

that is, the sum of any number of terms of this general form, where A, $a$ and $b$ are arbitrary.

Suppose that the ends of the wire are connected to apparatus, resistances, condensers, &c., in a given manner, and that the conditions thus imposed are expressed in an analytical form. Substitution in the general term of (4) will give rise to two equations—

$$\tan.\,(a + b) = \phi_1\,(a) \qquad \tan.\,b = \phi_2\,(a)$$

and between them we can eliminate $b$, obtaining

$$\tan. a = \phi(a)$$

These determine the admissible values of $a$ and $b$ consistent with the nature of the terminal connections; and if the electrical state of the system is known at any moment, the co-efficients A can be determined so that the right hand side of (4) expresses the potential of the line at that moment and subsequently.

If $v = \mathrm{U}$ when $t = 0$, where U is an arbitrary function of $x$, then will the general value of A be

$$\mathrm{A} = \frac{\frac{2}{l}\int_0^l \mathrm{U} \sin.\left(\frac{a x}{l} + b\right) d x}{1 - \cos.^2 a \frac{d}{d a} \phi(a)} \quad \ldots \quad \ldots \quad \ldots \quad (5.)$$

This is on the supposition that at $t = 0$ none of the energy of the system resided in the terminal apparatus. For example, if there is a condenser, it must be uncharged; otherwise, additional terms, which are easily found, must be added to the numerator of (5) the denominator remaining the same.

In fact

$$\mathrm{U} = \frac{2}{l} \Sigma \sin.\left(\frac{a x}{l} + b\right) \frac{\int_0^l \mathrm{U} \sin.\left(\frac{a x}{l} + b\right) d x}{1 - \frac{d}{d a} \tan.^{-1} \phi(a)}$$

is an identity. If the line be infinitely long, it becomes

$$f(x) = \frac{2}{\pi} \int_0^\infty d u \int_0^\infty d y \sin.(u x + b) \sin.(u y + b) f(y)$$

where $f(x)$ is any function of $x$ and $\sin. b = \cos. b \times \phi_2(u)$ where $\phi_2(u)$ is of the form

$$\phi_2(u) = h_1 u + h_3 u^3 + h_5 u^5 + \ldots$$

The above is an extended form of Fourier's Theorem as given in Treatises on the Integral Calculus.

8. With any number of wires we have

$$\gamma_1 = -\frac{1}{k_1} v'_1 ; \gamma_2 = -\frac{1}{k_2} v'_2, \text{ etc.} \ldots \quad \ldots \quad (6)$$

where $k_1$, $k_2$ ... are their resistances per unit of length. Therefore by eliminating $\gamma_1, \gamma_2 \dots$ between (2) and (6),

$$v''_1 = k_1 \dot{\rho}_1; \; v''_2 = k_2 \dot{\rho}_2; \text{ etc.} \quad \dots \quad \dots \quad (7.)$$

from which $\rho_1, \rho_2 \dots$ may be eliminated by means of (1), leading to

$$\left.\begin{array}{l} v''_1 = k_1 (c_1 \dot{v}_1 + c_{12} \dot{v}_2 + c_{13} \dot{v}_3 + \dots) \\ v''_2 = k_2 (c_{12} \dot{v}_1 + c_2 \dot{v}_2 + c_{23} \dot{v}_3 + \dots) \\ \cdot \quad \cdot \quad \cdot \quad \cdot \quad \cdot \quad \cdot \quad \cdot \quad \cdot \quad \cdot \end{array}\right\} \quad \dots \quad (8.)$$

the exact solutions of which for any given terminal conditions and given initial distributions may be readily obtained.

Beginning with the simple case of two wires of the same length, exactly similar, and at the same height from the ground, so that

$$c_1 = c_2 = c; \; k_1 = k_2 = k.$$

Here

$$v''_1 = c\,k\,\dot{v}_1 + c_{12}\,k\,\dot{v}_2; \; v''_2 = c\,k\,\dot{v}_2 + c_{12}\,k\,\dot{v}_1.$$

Now choose two other dependent variables $w_1$ and $w_2$, so that

$$v_1 = w_1 + w_2; \; v_2 = w_1 - w_2;$$

and substitute; then

$$w''_1 = (c + c_{12})\,k\,\dot{w}_1; \; w''_2 = (c - c_{12})\,k\,\dot{w}_2.$$

By this substitution we have got two equations of the same form as (3), that for a wire uninfluenced by the induction of others. Thus, imagine two fresh lines of capacities $c + c_{12}$ and $c - c_{12}$ per unit of length, which have *no* induction on one another, and let their potentials at any moment be half the sum and half the difference of those of the real lines at the same moment at corresponding points; let these imaginary lines discharge independently, then the potentials of the real lines at any subsequent moment will be the sum and the difference of what the potentials of the imaginary lines have then become.

For instance, if the first line had at $t = 0$ a steady current $E \div k\,l$ in it and potential $v = E\left(1 - \frac{x}{l}\right)$, while in the second line $v_2 = 0$, and the battery is then removed and both lines earthed at both ends, the subsequent potentials are

$$v_1 = \frac{\mathrm{E}}{\pi} \Sigma \frac{1}{n} \sin. \frac{n \pi x}{l} \left( e^{\mathrm{D}_1 t} + e^{\mathrm{D}_2 t} \right)$$

$$v_2 = \frac{\mathrm{E}}{\pi} \Sigma \frac{1}{n} \sin. \frac{n \pi x}{l} \left( e^{\mathrm{D}_1 t} - e^{\mathrm{D}_2 t} \right)$$

where

$$\mathrm{D}_1 = - \frac{n^2 \pi^2}{(c + c_{12}) k l^2}; \ \mathrm{D}_2 = - \frac{n^2 \pi^2}{(c - c_{12}) k l^2}$$

and the summations include all integral values of $n$ from 1 to $\infty$

At the end remote from the battery the currents are

$$\gamma_1 = \frac{\mathrm{E}}{k l} \Sigma (- \cos. n \pi) \left( e^{\mathrm{D}_1 t} + e^{\mathrm{D}_2 t} \right)$$

$$\gamma_2 = \frac{\mathrm{E}}{k l} \Sigma (- \cos. n \pi) \left( e^{\mathrm{D}_1 t} - e^{\mathrm{D}_2 t} \right).$$

The arrival curve of the current at the remote end of a li earthed at both ends and not exposed to induction from other wi is given in a readily accessible form in Professor Jenkin's *Electric and Magnetism*, with a table of the value of the current at differ times after contact was made. If the curve there given be dra on two different scales as regards time, viz., proportional to $c$ + and $c - c_{12}$ and from these two curves are constructed two n ones by making the ordinates of the latter equal to half the s and half the difference of the former, the latter will be the arri curves for the case just considered the big curve being the curr on line 1, and the smaller one, above the zero line, the induc arrival curve on line 2.

9. Again two wires, dissimilar, but with the same termi conditions. Here, by (8)

$$\left. \begin{aligned} v''_1 &= c_1 k_1 \dot{v}_1 + c_{12} k_1 \dot{v}_2 \\ v''_2 &= c_2 k_2 \dot{v}_2 + c_{12} k_2 \dot{v}_1 \end{aligned} \right\} \quad \ldots \quad \ldots \quad (9.)$$

To find elementary simultaneous solutions, eliminate either $v_1$ $v_2$; thus

$$(\nabla^2 - c_1 k_1 \mathrm{D}) (\nabla^2 - c_2 k_2 \mathrm{D}) v = c_{12}^2 k_1 k_2 \mathrm{D}^2 v$$

where $\nabla$ stands for $\frac{d}{dx}$ and D for $\frac{d}{dt}$. Let $\nabla^2$ be any numeri quantity, the above is an algebraical quadratic equation in whose roots are

$${}_{2}^{1} = \frac{\nabla^2}{2k_1k_2(c_1c_2 - c_{12}^2)}\left\{(c_1k_1 + c_2k_2) \pm \sqrt{(c_1k_1 + c_2k_2)^2 - 4k_1k_2(c_1c_2 - c_{12}^2)}\right\}$$

herefore

$$\left.\begin{aligned} v_1 &= \sin.\,(i\,\nabla x + b)\left(A\,e^{D_1 t} + B\,e^{D_2 t}\right) \\ v_2 &= \sin.\,(i\,\nabla x + b)\left(r_1 A\,e^{D_1 t} + r_2 B\,e^{D_2 t}\right) \end{aligned}\right\} \quad (10.)$$

here $i$ stands for $\sqrt{-1}$. A, B, $\nabla$ and $b$ are arbitrary, while $r_2$ are constants which may be found by inserting these solutions 0) in (9), giving

$$r_1 = \frac{\nabla^2 - c_1 k_1 D_1}{c_{12}\, k_1\, D_1}$$

$$r_2 = \frac{\nabla^2 - c_1 k_1 D_2}{c_{12}\, k_1\, D_2}.$$

The sum of an infinite number of elementary solutions such as ose in (10) may be made to represent initially any given stribution of potential in the wires, and also to satisfy the rminal conditions. The latter, when given, settle the admissible lues of $\nabla$ and $b$. $D_1$ and $D_2$ are known multiples of $\nabla$, and ally the value of $A + B$ and $r_1 A + r_2 B$, and therefore of A d B can be found by integration, so as to make the complete lutions represent any initial arbitrary potentials.

In the simple case of direct earth connection at both ends of oth lines, and $v_1 = E\left(1 - \frac{x}{l}\right)$, $v_2 = 0$, initially, we have $= 0$ and $i\,\nabla_n = \frac{n\,\pi}{l}$,

$$A_n + B_n = \frac{2\,E}{n\,\pi};\ r_1 A_n + r_2 B_n = 0$$

$$\therefore\ A_n = \frac{r_2}{r_2 - r_1}\,\frac{2\,E}{n\,\pi};\ B_n = -\frac{r_1}{r_2 - r_1}\,\frac{2\,E}{n\,\pi};$$

therefore

$$v_1 = \frac{2\,E}{\pi}\,\Sigma\,\frac{1}{n}\sin.\,\frac{n\,\pi\,x}{l}\left(r_2\,e^{D_1 t} - r_1\,e^{D_2 t}\right) \div (r_2 - r_1)$$

$$v_2 = \frac{2\,E}{\pi}\,\Sigma\,\frac{1}{n}\sin.\,\frac{n\,\pi\,x}{l}\left(e^{D_1 t} - e^{D_2 t}\right) \times \frac{r_1\,r_2}{r - r_1}$$

represent the potentials at time $t$.

10. It will be observed that the differential equations (8) each contain the differential co-efficients with respect to $t$ of the potentials of all the lines. But by a linear transformation, thus:—

$$\begin{aligned} v_1 &= w_1 + w_2 + w_3 + \dots \\ v_2 &= a_1 w_1 + a_2 w_2 + a_3 w_3 + \dots \\ v_3 &= b_1 w_1 + b_2 w_2 + b_3 w_3 + \dots \\ &\cdot \quad \cdot \quad \cdot \quad \cdot \quad \cdot \quad \cdot \quad \cdot \quad \cdot \quad \cdot \end{aligned}$$

and by properly determining the constants $a_1 \dots, b_1 \dots,$ equations (8) may be brought to the form

$$w''_1 = f_1 \dot{w}_1; \; w''_2 = f_2 \dot{w}_2; \text{ etc.}$$

each of which contains only one dependent variable, so that we may represent the actual potentials of the lines as linear functions of the potentials of an equal number of imaginary lines having no mutual influence, with the same terminal conditions, and charged initially in a known manner. But there is no advantage in commencing thus, the following process being simpler:—

Equations (8) may be written

$$\left.\begin{aligned} 0 &= p_{11} v_1 + p_{12} v_2 + p_{13} v_3 + \dots \\ 0 &= p_{21} v_1 + p_{22} v_2 + p_{23} v_3 + \dots \\ 0 &= p_{31} v_1 + p_{32} v_2 + p_{33} v_3 + \dots \\ &\cdot \quad \cdot \quad \cdot \quad \cdot \quad \cdot \quad \cdot \quad \cdot \quad \cdot \quad \cdot \end{aligned}\right\}$$

where $\quad p_{11} = -\nabla^2 + c_1 k_1 \text{D}; \; p_{12} = c_{12} k_1 \text{D}; \text{ etc.}$

By eliminating all the variables except one, we get for the equation of the potential of any line $\text{P}\, v = 0$, where P is the determinant

$$\text{P} = \begin{vmatrix} p_{11} & p_{12} & p_{13} & \cdots \\ p_{21} & p_{22} & p_{23} & \cdots \\ p_{31} & p_{32} & p_{33} & \cdots \end{vmatrix}$$

The terminal conditions being the same for all the lines, the elementary solutions will all contain the same function of $x$; then, assuming $\nabla^2$ to be a numerical quantity, the equation $\text{P} = 0$ is an algebraical equation in $\frac{\text{D}}{\nabla^2}$ of the same degree as the number of lines. Let the roots for D be $\text{D}_1, \text{D}_2, \dots$. These are all known multiples of $\nabla^2$, and the elementary solutions are

$$v_1 = \sin. (i \nabla x + b) \left\{ A e^{D_1 t} + B e^{D_2 t} + C e^{D_3 t} + \dots \right\},$$

$$v_2 = \sin. (i \nabla x + b) \left\{ r_1 A e^{D_1 t} + r_2 B e^{D_2 t} + \dots \right\},$$

$$v_3 = \sin. (i \nabla x + b) \left\{ s_1 A e^{D_1 t} + s_2 B e^{D_2 t} + \dots \right\},$$

etc. The quantities $r_1 \dots s_1 \dots$ may be found by inserting these solutions in equations (8). As before, the terminal conditions settle the different values of $\nabla$ and $b$; and integrations serve to determine A, B, C, ... so that the sum of infinite series of the above elementary solutions may represent the initial potentials.

11. But if the terminal conditions are not limited to be the same for all the lines at $x = 0$, and also all the same at $x = l$, the elementary solutions must be more general. First, for two lines.

Here

$$(\nabla^2 - c_1 k_1 D)(\nabla^2 - c_2 k_2 D) v = c_{12}^2 k_1 k_2 D^2 v$$

as before, for either line. Treating D as a constant number, this is a quadratic equation in $\nabla^2$, whose roots are

$$\nabla^2 = \frac{D}{2} \left\{ (c_1 k_1 + c_2 k_2) \pm \sqrt{(c_1 k_1 + c_2 k_2)^2 - 4 k_1 k_2 (c_1 c_2 - c_{12}^2)} \right\}$$

therefore an elementary solution is

$$\left.\begin{aligned} v_1 &= \left\{ (A_1 \sin. + B_1 \cos.) i \nabla_1 x + (A_2 \sin. + B_2 \cos.) i \nabla_2 x \right\} e^{Dt}, \\ v_2 &= \left\{ r_1 (A_1 \sin. + B_1 \cos.) i \nabla_1 x + r_2 (A_2 \sin. + B_2 \cos.) i \nabla_2 x \right\} e^{Dt} \end{aligned}\right\} \quad (11.)$$

where

$$r_1 = \frac{\nabla_1^2 - c_1 k_1 D}{c_{12} k_1 D}; \quad r_2 = \frac{\nabla_2^2 - c_1 k_1 D}{c_{12} k_1 D}$$

by inserting (11) in (9).

The four terminal conditions, viz., two for each line, give the ratios $A_1 : B_1 : A_2 : B_2$, and also furnish an equation for the admissible values of D.

We have thus the complete solutions

$$v_1 = \Sigma A u_1 e^{Dt}; \quad v_2 = \Sigma A u_2 e^{Dt}$$

where the functions $u_1$ and $u_2$ are completely known; also the values of D. If when $t = 0$, $v_1 = U_1$ and $v_2 = U_2$, these being arbitrary functions of $x$, we must find the values of the constants A so that

$$\left.\begin{aligned} U_1 &= \Sigma A u_1 \\ U_2 &= \Sigma A u_2 \end{aligned}\right\} \quad \dots \quad \dots \quad \dots \quad (12.)$$

This may be done by availing ourselves of the conjugate property possessed by two different possible distributions of potential and density. Let $u_{1i}$, $u_{2i}$ represent one, and $u_{1j}$, $u_{2j}$ represent another elementary distribution of potential, corresponding to $D_i$ and $D_j$ respectively. The first decays at one rate, the second at another, they may co-exist without interference, and their mutual potential energy is zero. If $E_{ij}$ is their mutual potential energy

$$E_{ij} = \int (u_{1i}\,\rho_{1j} + u_{2i}\,\rho_{2j})\,dx = \int (u_{1j}\,\rho_{1i} + u_{2j}\,\rho_{2i})\,dx$$

where $\rho_{1i}$, $\rho_{2i}$ are the densities corresponding to $u_{1i}$, $u_{2i}$ and $\rho_{1j}$, $\rho_{2j}$ those corresponding to $u_{1j}$, $u_{2j}$. This reciprocal property is an identity and does not depend on the distributions being independent. That $E_{ij} = 0$ for independent distributions may be thus proved. We have

$$u''_{1i} = D_i\,k_1\,\rho_{1i} \quad \ldots \quad (13.)_1 \qquad u''_{2i} = D_i\,k_2\,\rho_{2i} \quad \ldots \quad (13.)_3$$
$$u''_{1j} = D_j\,k_1\,\rho_{1j} \quad \ldots \quad (13.)_2 \qquad u''_{2j} = D_j\,k_2\,\rho_{2j} \quad \ldots \quad (13.)_4$$

by (7.) Therefore

$$E_{ij} = \int (u_{1i}\,\rho_{1j} + u_{2i}\,\rho_{2j})\,dx$$
$$= \frac{1}{D_j\,k_1}\int u_{1i}\,u''_{1j}\,dx + \frac{1}{D_j\,k_2}\int u_{2i}\,u''_{2j}\,dx$$

by $(13)_2$ and $(13)_4$,

$$= \frac{1}{D_j\,k_1}[u_{1i}\,u'_{1j} - u'_{1i}\,u_{1j}] + \frac{1}{D_j\,k_1}\int u''_{1i}\,u_{1j}\,dx$$
$$+ \frac{1}{D_j\,k_2}[u_{2i}\,u'_{2j} - u'_{2i}\,u_{2j}] + \frac{1}{D_j\,k_2}\int u''_{2i}\,u_{2j}\,dx$$

by double integration by parts,

$$= \frac{1}{D_j\,k_1}[u_{1i}\,u'_{1j} - u'_{1i}\,u_{1j}] + \frac{1}{D_j\,k_2}[u_{2i}u'_{2j} - u'_{2i}\,u_{2j}]$$
$$+ \frac{D_i}{D_j}\int u_{1j}\,\rho_{1i}\,dx + \frac{D_i}{D_j}\int u_{2j}\,\rho_{2i}\,dx$$

by $(13)_1$ and $(13)_3$. Therefore, by the reciprocal property

$$(D_j - D_i)\,E_{ij} = \frac{1}{k_1}[u_{1i}\,u'_{1j} - u'_{1i}\,u_{1j}]$$
$$+ \frac{1}{k_2}[u_{2i}\,u'_{2j} - u'_{2i}\,u_{2j}] \quad (14.)$$

The right-hand side is to be taken between limits, and vanishes when $i$ and $j$ are different if $\frac{u}{u'} =$ constant, that is, if the lines are put to earth at the ends, or insulated, or put to earth through mere resistances. When $i = j$, we find, by taking the limit and discarding $i$ and $j$

$$\int (u_1 \rho_1 + u_2 \rho_2)\, d x = \frac{1}{2\, n\, k_1}\left[u'_1 \frac{d\, u_1}{d\, n} - u_1 \frac{d\, u'_1}{d\, n}\right] + \frac{1}{2\, n\, k_2}\left[u'_2 \frac{d\, u_2}{d\, n} - u_2 \frac{d\, u'_2}{d\, n}\right] \quad (15.)$$

where $n^2 = -\,\mathrm{D}$. By applying (14) and (15) to (12) the value of any co-efficient A is found to be

$$\mathrm{A} = \frac{\int \mathrm{U}_1\, u_1\, \rho_1\, d\, x + \int \mathrm{U}_2\, u_2\, \rho_2\, d\, x}{\frac{1}{2\, n\, k_1}\left[u'_1 \frac{d\, u_1}{d\, n} - u_1 \frac{d\, u'_1}{d\, n}\right] + \frac{1}{2\, n\, k_2}\left[u'_2 \frac{d\, u_2}{d\, n} - u_2 \frac{d\, u'_2}{d\, n}\right]}$$

the limits for the integrations being 0 and $l$, and the quantities in square brackets being taken between the same. The form of A requires modification when $\frac{u}{u'}$ not $=$ constant.

12. The extension to any number of wires $m$ is obvious. In § 10, in the equation $\mathrm{P} v = 0$, let D be any numerical quantity, then $\mathrm{P} = 0$ is an algebraical equation in $\frac{\nabla^2}{\mathrm{D}}$ of the $m^{th}$ degree. Let its roots for $\nabla^2$ be $\nabla_1^2$, $\nabla_2^2$, etc., then

$$v_1 = u_1\, e^{\mathrm{D}\, t}\,;\; v_2 = u_2\, e^{\mathrm{D}\, t}\,;\; v_3 = u_3\, e^{\mathrm{D}\, t}\,;\; \text{etc.},$$

where

$$u_1 = (\mathrm{A}_1 \sin. + \mathrm{B}_1 \cos.)\, i\, \nabla_1 x + (\mathrm{A}_2 \sin. + \mathrm{B}_2 \cos.)\, i\, \nabla_2 x + (\mathrm{A}_3 \sin. + \mathrm{B}_3 \cos.)\, i\, \nabla_3 x + \ldots$$

$$u_2 = r_1\, (\mathrm{A}_1 \sin. + \mathrm{B}_1 \cos.)\, i\, \nabla_1 x + r_2\, (\mathrm{A}_2 \sin. + \mathrm{B}_2 \cos.)\, i\, \nabla_2 x + r_3\, (\mathrm{A}_3 \sin. + \mathrm{B}_3 \cos.)\, i\, \nabla_3 x + \ldots$$

etc., etc.

is a system of elementary solutions; the ratios $r_1$, ... being found by insertion in (8). The constants $\mathrm{A}_1\, \mathrm{B}_1$ ... are $2m$ in number. The terminal conditions are also $2m$ in number, and serve to determine the $2m-1$ ratios $\mathrm{A}_1 : \mathrm{B}_1 : \mathrm{A}_2 : \mathrm{B}$ : ... and give besides an equation for D. Thus the complete solutions are

$$v_1 = \Sigma\, A\, u_1\, e^{Dt};\ v_2 = \Sigma\, A\, u_2\, e^{Dt};\ \text{etc.} \quad \ldots \quad (16.)$$

where $u_1, u_2 \ldots$ and the values of D are completely known. The constants A may be found by the conjugate property, which is now more complex, though essentially the same. $u_{1i}\ u_{2i}\ u_{3i} \ldots$ and $u_{1j}\ u_{2j}\ u_{3j} \ldots$ being two different independent potential distributions, and $\rho_{1i} \ldots,\ \rho_{1j} \ldots,$ the corresponding densities, the reciprocal property is

$$E_{ij} = \int (u_{1i}\,\rho_{1j} + u_{2i}\,\rho_{2j} + u_{3i}\,\rho_{3j} + \ldots)\, dx$$
$$= \int (u_{1j}\,\rho_{1i} + u_{2j}\,\rho_{2i} + u_{3j}\,\rho_{3i} + \ldots)\, dx$$

and the conjugate property may be shown in a similar manner to before to be

$$(D_j - D_i)\, E_{ij} = \frac{1}{k}[u_{1i}\, u'_{1j} - u'_{1i}\, u_{1j}] + \frac{1}{k_2}[u_{2i}\, u'_{2j} - u'_{2i}\, u_{2j}] + \quad \ldots \quad \ldots \quad (17.)$$

Thus $E_{ij} = 0$ when $\dfrac{u}{u'} =$ constant. And

$$\int (u_{1i}\,\rho_{1i} + u_{2i}\,\rho_{2i} + u_{3i}\,\rho_{3i} + \ldots)\, dx$$
$$= \int (c_1\, u_1^2 + c_2\, u_2^2 + c_3\, u_3^2 + \ldots + 2\, c_{12}\, u_1\, u_2 + 2\, c_{13}\, u_2\, u_3 + \ldots)\, dx$$
$$= \frac{1}{2\, n\, k_1}\left[u'_1\, \frac{d\, u_1}{d\, n} - u_1\, \frac{d\, u'_1}{d\, n}\right] + \text{similar terms with other suffixes} \quad \ldots \quad (18.)$$

where $n^2 = -D$.

The left-hand member is twice the potential energy of $u_1, u_2, u_3, \ldots$ By these properties (17) and (18), applied to (15), the constants A may be found so as to make $v_1 = U_1$, $v_2 = U_2$, $v_3 = U_3$, &c., when $t = 0$, where $U_1 \ldots$ are arbitrary functions of $x$.

13. In paragraphs 7 to 12 inclusive only electro-static induction has been taken into account. A considerably greater complication arises when we attempt to fully exhibit the joint action of both the inductions. We have still the fundamental equations of continuity,

$$\gamma'_1 + \rho_1 = 0\,;\ \gamma'_2 + \rho_2 = 0\,;\ \text{etc.}$$

and also the same relations between potential and density,

$$\rho_1 = c_1\, v_1 + c_{12}\, v_2 + c_{13}\, v_3 + \ldots$$

etc., but the equations connecting the potential and current are now no longer

$$\gamma_1 = -\frac{1}{k_1}\, v_1^1\,;\ \gamma_2 = -\frac{1}{k_2}\, v_2^1\,;\ \text{etc.}$$

but are of the much more complex form:

$$\left.\begin{array}{l} -v_1^1 = k_1\,\gamma_1 + s_1\,\dot{\gamma}_1 + s_{12}\,\dot{\gamma}_2 + s_{13}\,\dot{\gamma}_3 + \ldots \\ -v_2^1 = k_2\,\gamma_2 + s_{12}\,\dot{\gamma}_1 + s_2\,\dot{\gamma}_2 + s_{23}\,\dot{\gamma}_3 + \ldots \\ -v_3^1 = k_3\,\gamma_3 + s_{13}\,\dot{\gamma}_1 + s_{23}\,\dot{\gamma}_2 + s_3\,\dot{\gamma}_3 + \ldots \\ \cdot\quad\cdot\quad\cdot\quad\cdot\quad\cdot\quad\cdot\quad\cdot\quad\cdot\quad\cdot\quad\cdot\quad\cdot\quad\cdot\quad\cdot\quad\cdot \end{array}\right\} \qquad (19.)$$

Here $s_1$, $s_2$, $s_3$, . . . are the co-efficients of self-induction (or electro-magnetic capacity) of the wires per unit of length, while $s_{12}$ is the co-efficient of mutual capacity of 1 and 2 per unit of length, $s_{23}$ the same for 2 and 3, etc., and $\gamma_1\ \gamma_2$ . . . are the currents in the wires. (*See* vol. ii., Maxwell's *Electricity*.) The impressed E.M.F. $-v$ in wire 1 is not only employed in maintaining the current in that wire, but also in varying the currents in all the rest.

Eliminating the currents, we have

$$\left.\begin{array}{l} v_1^{11} = k_1\,\dot{\rho}_1 + s_1\,\ddot{\rho}_1 + s_{12}\,\ddot{\rho}_2 + \ldots \\ v_2^{11} = k_2\,\dot{\rho}_2 + s_{12}\,\ddot{\rho}_1 + s_2\,\ddot{\rho}_2 + \ldots \\ v_3^{11} = k_3\,\dot{\rho}_3 + s_{13}\,\ddot{\rho}_1 + s_{23}\,\ddot{\rho}_2 + \ldots \\ \cdot\quad\cdot\quad\cdot\quad\cdot\quad\cdot\quad\cdot\quad\cdot\quad\cdot\quad\cdot\quad\cdot\quad\cdot \end{array}\right\} \ldots\quad\ldots\quad (20.)$$

and since the densities are linear functions of the potentials, the former may be eliminated, giving rise to a set of simultaneous partial differential equations, each containing all the dependent variables $v_1$, . . . and $\dfrac{d^2}{dx^2}, \dfrac{d}{dt}$, and $\dfrac{d^2}{dt^2}$. By eliminating the potentials, the resulting equations in the densities are somewhat simpler as regards the co-efficients.

Beginning, for more completeness, with the case of a single wire, we have

$$v^{11} = c\,k\,\dot{v} + c\,s\,\ddot{v} \quad \ldots \quad \ldots \quad \ldots \quad (21.)$$

or
$$\nabla^2 v = (c k \mathrm{D} + c s \mathrm{D}^2)\, v$$
whose general solution is
$$v = \Sigma \sin. (i \nabla x + b)\ (\mathrm{A}\, e^{\mathrm{D}_1 t} + \mathrm{B}\, e^{\mathrm{D}_2 t})$$
where $\mathrm{D}_1$ and $\mathrm{D}_2$ are the roots of
$$c s \mathrm{D}^2 + c k \mathrm{D} - \nabla^2 = o$$
and $\nabla^2$ is any numerical quantity. That is
$$\mathrm{D}_{\frac{1}{2}} = \frac{1}{2 c s}\left(- c k \pm \sqrt{c^2 k^2 + 4 c s \nabla^2}\right)$$

For simplicity, let the wire be earthed at both ends, then $i \nabla = \frac{n \pi}{l}$, where $n$ is any integer, and $b = 0$, so the elementary solution is
$$v = \sin. \frac{n \pi x}{l} (\mathrm{A}\, e^{\mathrm{D}_1 t} + \mathrm{B}\, e^{\mathrm{D}_2 t});$$
or, if $\mathrm{D}_1$ and $\mathrm{D}_2$ are imaginary,
$$v = \sin. \frac{n \pi x}{l}\, e^{-p t}\, (\mathrm{A} \sin. + \mathrm{B} \cos.)\, q t$$
where
$$p = \frac{k}{2 s};\ q = \frac{k}{s} \sqrt{\frac{n^2 \pi^2}{l^2} \cdot \frac{s}{c k^2} - \frac{1}{4}}$$
The latter form will be used preferably. If $q$ be imaginary, $\mathrm{D}_1$ and $\mathrm{D}_2$ are real and negative, and the simple harmonic distribution of potential $\sin. \frac{n \pi x}{l}$ decreases asymptotically towards zero. But if $q$ be real it oscillates at a rate proportional to $q$, the amplitude of the oscillations decreasing asymptotically towards zero at a rate proportional to $p$. Whether $v$ is oscillatory or not depends on the values of $l$, $n$, $s$, $c$, and $k$ Let $c = \cdot 02$ microf. per mile, $k = 15$ ohms per mile. The value of $s$ for iron wire depends mainly on the quality of the iron, and is therefore rather indefinite in the absence of actual measurement. Let $s = 15 \times 10^6$ per mile (electro-magnetic units). Then
$$\frac{s}{k} = \frac{15 \times 10^6}{15 \times 10^9} = 10^{-3} \text{ sec.},$$
and $\qquad c k = \cdot 02 \times 10^{-15} \times 15 \times 10^9 = 3 \times 10^{-7}$ sec.;
therefore
$$q = 10^3 \sqrt{\frac{n^2}{l^2} \frac{10^5}{3} - \frac{1}{4}} \text{ approximately.}$$

Now the first value of $n$ is 1, so for the first term to be oscillatory $l^2 < \frac{4}{3}\, 10^6$, or $l < 365$ miles. Therefore if the length of the line is less than 365 miles, all the terms are oscillatory.

For 100 miles

$$q = 10^3 \sqrt{\frac{10}{3}\, n^2 - \frac{1}{4}}$$

The period of an oscillation is $\frac{2\pi}{q}$; for the first term this amounts to $\frac{2\pi}{1755}$ second, or about $\frac{1}{280}$ second. But since $\frac{s}{k} = \frac{1}{1000}$ second, the amplitude falls very quickly, the second oscillation being small compared with the first. It is only on short lines that there can be many oscillations without much diminution of amplitude.

The physical reason of the oscillation will be readily understood from §5. Suppose that at $t = 0$, $v = \sin. \frac{\pi x}{l}$, so that the density is $\rho = c \sin. \frac{\pi x}{l}$, that the charge is at rest at that time, and is then left to itself. The E.M.F. of the charge is at first $-\frac{\pi}{l} \cos. \frac{\pi x}{l}$; this sets the charge in motion symmetrically from the centre. Self-induction retards the outward flow at first, but once in motion the current requires force to stop it. Therefore the current does not cease when the line is discharged, but continues, thus producing a negative electrification similar to the first positive, though smaller, and the E.M.F. of this $-$ charge brings the current to rest. Then the current sets in the other way producing similarly a $+$ charge, and this goes on until the resistance of the line uses up all the original energy.

Suppose that the line is originally free from charge and connected to earth at both ends, and that at the time $t = 0$ an electromotive-force E is put on at $x = 0$. The course of the potential is then

$$v = \mathrm{E}\left(1 - \frac{x}{l}\right) - \frac{2\,\mathrm{E}}{\pi} \Sigma \frac{1}{n} \sin. \frac{n \pi x}{l}\, e^{-pt} \left(\frac{p}{q} \sin. + \cos. \right) q\, t \qquad (22.)$$

And of the current

$$\gamma = \frac{\mathrm{E}}{k\, l}\left(1 - e^{-2pt}\right) + \frac{2\,\mathrm{E}}{k\, l} \Sigma \cos. \frac{n \pi x}{l} \cdot \frac{2\, p}{q}\, e^{-pt} \sin. q\, t \ldots (23.)$$

These formulæ are not very intelligible without some study. A rough idea of their meaning may however be obtained without calculating them exactly. When the line is quite short the $\frac{1}{4}$ under the radical sign in the value of $q$ is small compared with the preceding term, and $\frac{p}{q}$ is also small compared with 1; therefore, by neglecting $\frac{p}{q}$ sin. $q\,t$ in (22), and taking $q = \frac{n\,\pi}{l\sqrt{c\,s}}$ by neglecting the $\frac{1}{4}$, that equation becomes

$$v = \mathrm{E}\left(1 - \frac{x}{l}\right) - \frac{2\,\mathrm{E}}{\pi}\,\Sigma\,\frac{1}{n}\sin.\frac{n\,\pi\,x}{l}\,e^{-p\,t}\cos.\frac{n\,\pi\,t}{l\sqrt{c\,s}} \qquad (24.)$$

Now it may be shown that if we cancel the factor $e^{-p\,t}$ common to all the terms in the summation in (24), that

$$v = 0 \text{ from } t = 0 \text{ to } t = x\sqrt{s\,c}$$

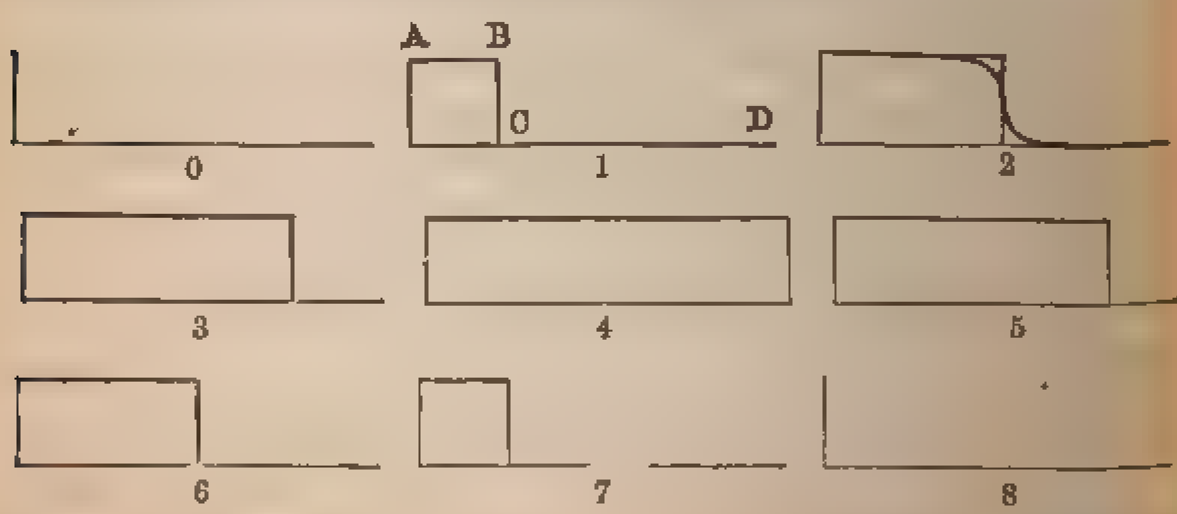
and $$v = \mathrm{E} \text{ from } t = x\sqrt{s\,c} \text{ to } t = l\sqrt{s\,c}$$

Or,

$$v = \mathrm{E} \text{ when } 0 < x < \frac{t}{\sqrt{s\,c}}$$

$$v = 0 \text{ when } \frac{t}{\sqrt{s\,c}} < x < l$$

This is best interpreted graphically. Divide the period $2\,l\sqrt{s\,c}$ into say eight parts equal to $\tau$, let abscissæ represent distance along the line, and ordinates the potential, then the following show the progress of the potential during one period.




The potential E here travels to and fro along the line with uniform velocity $\frac{1}{\sqrt{s\,c}}$. At $x = \frac{l}{2}$, $v = 0$ from $t = 0$ to $t = 2\tau$,

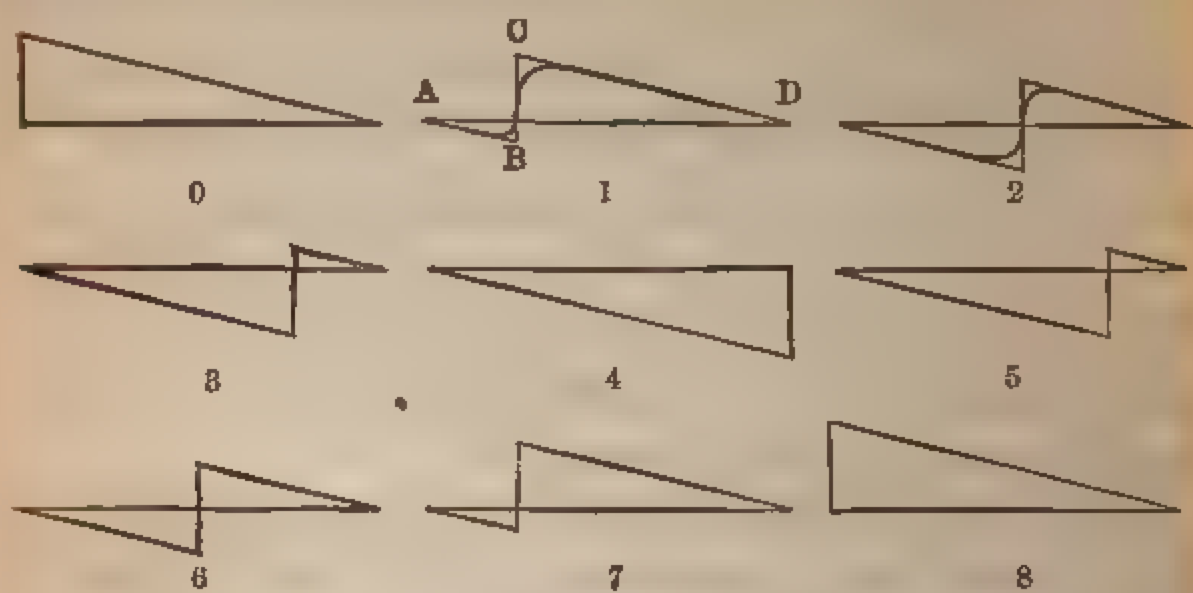
is then E from $t = 2\tau$ to $t = 6\tau$, then 0 from $t = 6\tau$ to $t = 10\tau$, and so on, thus being alternately E and 0 for equal periods, its mean value being $\frac{E}{2}$. At $x = \frac{l}{4}$, $v = 0$ from $t = 0$ to $t = \tau$, is then E from $t = \tau$ to $t = 7\tau$, then 0 from $t = 7\tau$ to $t = 9\tau$, and so on, being three times as long at E as at 0, so that its mean value is $\frac{3E}{4}$. At $x = \frac{3l}{4}$, $v$ is also alternately 0 and E, but is three times as long at 0 as at E, its mean value being $\frac{E}{4}$. Similarly for any other point, so that the mean value of $v$ is $E\left(1 - \frac{x}{l}\right)$, what it ultimately becomes in the real case.

Now the abrupt discontinuities are due to the assumptions made, so that to pass to the real case we must first of all round off the sharp corners B and C, and slightly curve the straight lines, as shown in the figure marked 2. And next, we must introduce the factor $e^{-pt}$. The effect of this is to make the summation in (24), *i.e.*, the amount by which $v$ depart from $E\left(1 - \frac{x}{l}\right)$, the final potential, decrease rapidly with the time. As the wave progresses the upper portion continuously falls, while the lower portion continuously rises, and the double turn in the curve becomes less prominent, and is finally wiped out altogether, A B C D thus becoming ultimately a straight line joining A and D, representing $v = E\left(1 - \frac{x}{l}\right)$ the final state.

The rapidity with which the wave decreases being proportional to $p$ is the same for any length; the velocity of the wave is also constant. But as the time the wave takes to reach the distant end is $l\sqrt{sc}$, the longer the line is, the less does the wave become before it has made a single journey from 0 to $l$. For 100 miles, $p = \frac{2s}{k} = \frac{1}{500}$ second, (see above) and $l\sqrt{sc} = \frac{\sqrt{3}}{1000}$, so that even on its first journey the wave is greatly diminished. For 10 miles $l\sqrt{sc} = \frac{\sqrt{3}}{10000}$, so that the wave may make 10 journeys before it decays as much as in one journey on a line of 100 miles.

On removing E, there is a similar wave travelling to and fro

whilst $v$ falls from $\mathrm{E}\left(1-\frac{x}{l}\right)$ to 0. Corresponding to the former figures, we have the following:—




showing the changes during the period $2\,l\sqrt{s\,c}$, divided into eight equal intervals. Here $v$ at any point $x$ is alternately $\mathrm{E}\left(1-\frac{x}{l}\right)$ and $-\frac{\mathrm{E}x}{l}$, the times during which it has these values being such that the mean value of $v$ is 0. Thus at $x=\frac{l}{4}$, $v$ is three times as long at $-\frac{\mathrm{E}}{4}$ as at $+\frac{3\mathrm{E}}{4}$, and at $x=\frac{3l}{4}$, $v$ is three times as long at $\frac{\mathrm{E}}{4}$ as at $-\frac{3\mathrm{E}}{4}$. As before, the lines A B C D must be rounded, and the wave supposed to diminish rapidly as it progresses.

The current-wave may be similarly treated. Neglecting the $\frac{1}{2}$ in $q$ the summational part of (23) becomes

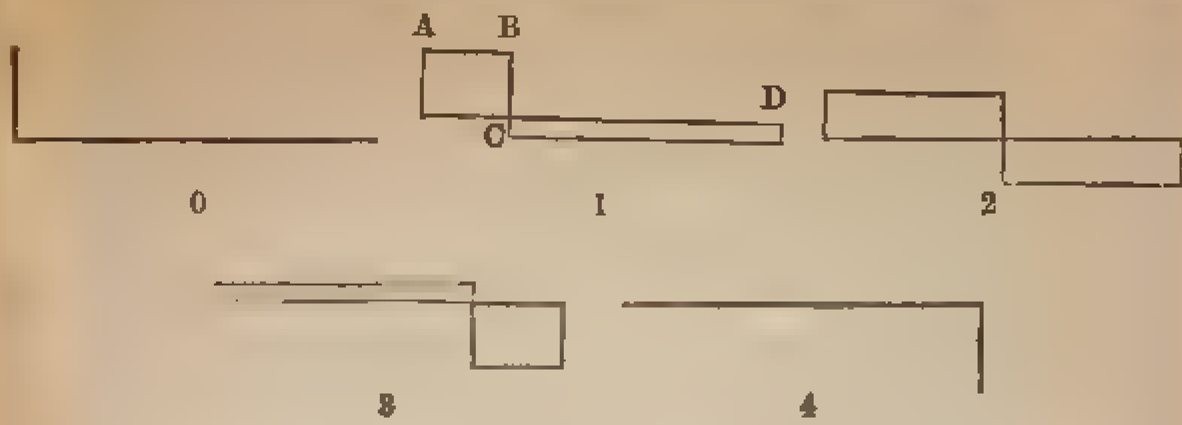
$$\frac{2\,\mathrm{E}}{\pi}\sqrt{\frac{c}{s}}\,\Sigma\,\frac{1}{n}\cos.\,\frac{n\,\pi\,x}{l}\,\epsilon^{-p\,t}\sin.\,\frac{n\,\pi\,t}{l\sqrt{s\,c}}$$

and cancelling the factor $\epsilon^{-p\,t}$, this represents

$$\mathrm{E}\sqrt{\frac{c}{s}}\left(1-\frac{t}{l\sqrt{s\,c}}\right)\text{ from }x=0\text{ to }x=\frac{t}{\sqrt{s\,c}}$$

and $$-\,\mathrm{E}\sqrt{\frac{c}{s}}\,\frac{t}{l\sqrt{s\,c}}\qquad\text{from }x=\frac{t}{\sqrt{s\,c}}\text{ to }x=l.$$

The changes during the semi-period $l\sqrt{s\,c}$ are shown in the following, where the ordinates now represent current.




In the second semi-period, 5 is the same as 3, 6 the same as 2, 7 the same as 1, and 8 the same as 0. As before, the lines A B C D must be rounded at B and C, and the wave as it progresses supposed to diminish rapidly. This wave is superimposed on the steadily rising current $\frac{E}{k\,l}(1 - e^{-2pt})$. As the latter is increasing in strength the oscillations are decreasing, and the final current is $\frac{E}{k\,l}$.

The ratio of the maximum strength of the current-wave to the final current is $k\,l\sqrt{\frac{c}{s}} = \frac{l\sqrt{3}}{100}$. For 100 miles this is 1·73, for 10 miles ·173, varying as the length of the line. The wave is strongest at the moment of starting from $x = 0$, where it may be taken to represent the well-known *charge* at the moment contact is made, which varies as the length of the line.

It should be remembered that this mode of representation applies to short lines only. On long lines the oscillations are practically confined to the battery end.

14. The simplest case for two wires is when they are alike, that is

$$c_1 = c_2 = c\,;\ k_1 = k_2 = k\,;\ s_1 = s_2 = s\,;$$

and their terminal conditions are the same. By (20) our equations are

$$v_1'' = c\,k\,\dot{v}_1 + (c\,s + c_{12}\,s_{12})\,\ddot{v}_1 + c_{12}\,k\,v_2 + (c\,s_{12} + c_{12}\,s)\,\ddot{v}_2$$

$$v_2'' = c\,k\,\dot{v}_2 + (c\,s + c_{12}\,s_{12})\,\ddot{v}_2 + c_{12}\,k\,\dot{v}_1 + (c\,s_{12} + c_{12}\,s)\,\ddot{v}$$

Here we may separate the dependent variables by the transformation

$$v_1 = w_1 + w_2\,;\ v_2 = w_1 - w_2:$$

so that the resulting equations

$$w_1^{11} = (c + c_{12}) \{k \dot{w}_1 + (s + s_{12}) \ddot{w}_1\}$$
$$w_2^{11} = (c - c_{12}) \{k \dot{w}_2 + (s - s_{12}) \ddot{w}_2\}$$

each contain only one dependent variable. These are what we should find for two wires of electrostatic capacity $c + c_{12}$ and $c - c_{12}$ and electro-magnetic capacity $s + s_{12}$ and $s - s_{12}$, with the same resistance as the real wires, having no mutual inductive action; and elementary solutions are

$$w_1 = \sin.\ (n x + b)\ e^{-p_1 t}\ (A_1 \sin.\ q_1 t + B_1 \cos.\ q_1 t)$$
$$w_2 = \sin.\ (n x + b)\ e^{-p_2 t}\ (A_2 \sin.\ q_2 t + B_2 \cos.\ q_2 t)$$

where $-p_1 + i q_1$ and $-p_2 \pm i q_2$ are the roots of

$$(c + c_{12})(s + s_{12}) D_1^2 + (c + c_{12}) k D_1 + n^2 = 0$$
$$(c - c_{12})(s - s_{12}) D_2^2 + (c - c^{12}) k D_2 + n^2 = 0$$

and $n$ is any numerical quantity. Or,

$$p_1 = \frac{k}{2(s + s_{12})};\ q_1 = \frac{\sqrt{4 n^2 \frac{s + s_{12}}{c + c_{12}} - k^2}}{2(s + s_{12})}$$

and $p_2\ q_2$ are found by changing the signs of $s_{12}$ and $c_{12}$ in $p_1$ and $q_1$. The solutions in the last paragraph may be immediately applied. Thus if both wires are to earth at both ends and E is applied to the first at $x = 0$ we shall have

$$w_1 = \frac{E}{2}\left(1 - \frac{x}{l}\right) - \frac{E}{\pi} e^{-p_1 t} \Sigma \frac{1}{n} \sin.\ \frac{n \pi x}{l} \left(\frac{p_1}{q_1} \sin. + \cos.\right) q_1 t$$
$$w_2 = \frac{E}{2}\left(1 - \frac{x}{l}\right) - \frac{E}{\pi} e^{-p_2 t} \Sigma \frac{1}{n} \sin.\ \frac{n \pi x}{l} \left(\frac{p_2}{q_2} \sin. + \cos.\right) q_2 t$$

for the imaginary wires, and $v_1$, $v_2$, the potentials of the real wires will be the sum and difference of $w_1$ and $w_2$. And if $\eta_1\ \eta_2$ are the currents corresponding to $w_1$ and $w_2$

$$\eta_1 = \frac{E}{2 k l}(1 - e^{-2 p_1 t}) + \frac{E}{k l} e^{-p_1 t} \Sigma \cos.\ \frac{n \pi x}{l} \frac{2 p_1}{q_1} \sin.\ q_1 t$$
$$\eta^2 = \frac{E}{2 k l}(1 - e^{-2 p_2 t}) + \frac{E}{k l} e^{-p_2 t} \Sigma \cos.\ \frac{n \pi x}{l} \frac{2 p_2}{q_2} \sin.\ q_2 t$$

and $\gamma_1\ \gamma_2$ will be the sum and difference of $\eta_1$ and $\eta_2$.

15. More generally, if the lines are not exactly similar,

$$v_1^{11} = a_1 \dot{v}_1 + b_1 \ddot{v}_1 + d_1 \dot{v}_2 + f_1 \ddot{v}_2$$
$$v_2^{11} = a_2 \dot{v}_2 + b_2 \ddot{v}_2 + d_2 \dot{v}_1 + f_2 \ddot{v}_1$$

where $a_1$, $b_1$, $d_1$, $f_1$, . . . may be found by inspecting (2) and (8). If the terminal conditions are the same, assume $\frac{d^2}{dx^2} = -n^2$, then

$$v_1 = \sin.\,(n\,x + b)\,(\mathrm{A}\,e^{\mathrm{D}_1 t} + \mathrm{B}\,e^{\mathrm{D}_2 t} + \mathrm{C}\,e^{\mathrm{D}_3 t} + \mathrm{D}\,e^{\mathrm{D}_4 t})$$
$$v_2 = \sin.\,(n\,x + b)\,(r_1\,\mathrm{A}\,e^{\mathrm{D}_1 t} + r_2\,\mathrm{B}\,e^{\mathrm{D}_2 t} + r_3\,\mathrm{C}\,e^{\mathrm{D}_3 t} + r_4\,\mathrm{D}\,e^{\mathrm{D}_4 t})$$

where $\mathrm{D}_1$ . . . are the roots of

$$(n^2 + a_1\,\mathrm{D} + b_1\,\mathrm{D}^2)\,(n^2 + a_2\,\mathrm{D} + b_2\,\mathrm{D}^2) = (d_1\,\mathrm{D} + f_1\,\mathrm{D}^2)\,(d_2\,\mathrm{D} + f_2\,\mathrm{D}^2),$$

$r_1$ is given by

$$0 = n^2 + a_1\,\mathrm{D}_1 + b_1\,\mathrm{D}_1^2 + r_1\,(d_1\,\mathrm{D}_1 + f_1\,\mathrm{D}_1^2)$$

and $r_2$ $r_3$ $r_4$ by similar equations containing $\mathrm{D}_2$ $\mathrm{D}_3$ $\mathrm{D}_4$ instead of $\mathrm{D}_1$.

After finding the admissible values of $n$ and $b$ from the terminal conditions, $\mathrm{D}_1$ . . . are known in terms of $n$, and by means of four integrations A, B, C, D, may be determined so as to make the sum of an infinite series of elementary solutions represent initially arbitrary distributions of potential and current.

16. Still more generally, any number $m$ of wires with the same terminal conditions. The result of eliminating from equations (20) all the variables but one is $\mathrm{P}v=0$, (see § 10); the elements of P are now

$$\begin{aligned}
p_{11} &= -\nabla^2 + c_1\,k_1\,\mathrm{D} + (c_1\,s_1 + c_{12}\,s_{12} + c_{13}\,s_{13} + c_{14}\,s_{14} + \ldots)\,\mathrm{D}^2 \\
p_{12} &= c_{12}\,k_1\,\mathrm{D} + (c_{12}\,s_1 + c_2\,s_{12} + c_{23}\,s_{13} + c_{24}\,s_{14} + \ldots)\,\mathrm{D}^2 \\
p_{13} &= c_{13}\,k_1\,\mathrm{D} + (c_{13}\,s_1 + c_{23}\,s_{12} + c_3\,s_{13} + c_{34}\,s_{14} + \ldots)\,\mathrm{D}^2 \\
&\cdot \quad \cdot \quad \cdot \quad \cdot \quad \cdot \quad \cdot \quad \cdot \quad \cdot \quad \cdot \\
p_{21} &= c_{12}\,k_2\,\mathrm{D} + (c_1\,s_{12} + c_{12}\,s_2 + c_{13}\,s_{23} + c_{14}\,s_{24} + \ldots)\,\mathrm{D}^2 \\
p_{22} &= -\nabla^2 + c_2\,k_2\,\mathrm{D} + (c_{12}\,s_{12} + c_2\,s_2 + c_{23}\,s_{23} + c_{24}\,s_{24} + \ldots)\,\mathrm{D}^2 \\
p_{23} &= c_{23}\,k_2\,\mathrm{D} + (c_{13}\,s_{12} + c_{23}\,s_2 + c_3\,s_{23} + c_{34}\,s_{24} + \ldots)\,\mathrm{D} \\
&\cdot \quad \cdot \quad \cdot \quad \cdot \quad \cdot \quad \cdot \quad \cdot \quad \cdot \quad \cdot
\end{aligned}$$

etc.

where $\nabla$ stands for $\frac{d}{dx}$ and D for $\frac{d}{dt}$. Putting $-\nabla^2 = n^2$, any numerical quantity, $\mathrm{P}=0$ is an algebraical equation in D of the $2m^{th}$ degree. Let its roots be $\mathrm{D}_1$ $\mathrm{D}_2$ . . . , then a system of simultaneous elementary solutions is

$$v_1 = \sin. (n x + b) (A_1 e^{D_1 t} + A_2 e^{D_2 t} + A_3 e^{D_3 t} + ...)$$
$$v_2 = \sin. (n x + b) (B_1 e^{D_1 t} + B_2 e^{D_2 t} + B_3 e^{D_3 t} + ...)$$
$$v_3 = \sin. (n x + b) (C_1 e^{D_1 t} + C_2 e^{D_2 t} + C_3 e^{D_3 t} + ...)$$
$$. \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad .$$

Here the ratios $A_1 : B_1 : C_1 : \ldots$ will be settled by the equations

$$0 = p_{11} A_1 + p_{12} B_1 + p_{13} C_1 + \ldots$$
$$0 = p_{21} A_1 + p_{22} B_1 + p_{23} C_1 + \ldots$$
$$0 = p_{31} A_1 + p_{32} B_1 + p_{33} C_1 + \ldots$$
$$. \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad .$$

where in the values of $p_{11} \ldots$ are substituted $-n^2$ for $\nabla^2$ and $D_1$ for D. Similarly the ratios $A_2 : B_2 : C_2 : \ldots$ by similar equations with $D_2$ for D instead of $D_1$. So after finding the admissible values of $n$ and $b$ from the terminal conditions, the elementary solutions above only contain $2m$ arbitrary constants. Therefore, as there are $m$ lines, $2m$ integrations in the ordinary manner, viz., two for every line, will determine the values of these constants for any term in the complete solution. The given data may be the initial values of $v$ and $\dot{v}$, or of $v$ and $\gamma$, or of something equivalent, for every line.

17. The influence of defective insulation will now be briefly considered. As every English telegraphist knows, on long lines in very bad weather the leakage is so great that hardly any current is received from the sending station, sometimes putting a stop to communication altogether. But although leakage when carried too far, is a great evil, it nevertheless has its good points and important practical uses. It quickens the signalling, or would do so were the speeds attained anything like what the amount of inductive retardation due to the lines would admit of (such speeds as can be got with Bain's recorder), which is far from being the case with the instruments in present use. But leakage has still its value, viz., in reducing the magnitude of the foreign currents in receiving instruments due to the induction of neighbouring wires. Charging one line, and so raising the potential of a parallel one, causes a flow of electricity from the latter to earth. Now defective insulation not only makes the inducing charge less, and therefore the induced charge less, but also allows a portion of the latter to

pass between earth and line through the poles direct, instead of through the receiving instruments.

When a line is permanently charged, the equation of its potential is

$$v'' = h^2 v,$$

where $h^2 = \frac{k}{i}$, and $k$ and $i$ are the resistances per unit of length of the wire and its insulation. The general solution being

$$v = A e^{hx} + B e^{-hx},$$

if the potential is that produced by an E.M.F., E at $x = 0$, and the wire is earthed at $x = l$, its potential is then

$$v = E \cdot \frac{e^{h(l-x)} - e^{-h(l-x)}}{e^{hl} - e^{-hl}},$$

and the total charge is therefore

$$\int_0^l v c\, dx = \frac{E c l}{2} \cdot \frac{2}{hl} \cdot \frac{e^{hl} + e^{-hl} - 2}{e^{hl} - e^{-hl}}$$

instead of $\frac{E c l}{2}$ when $h = 0$ and perfect insulation.

The influence of leakage is of course greatest on long lines. Let $l = 400$ miles, $k = 15$ ohms, and $i = 1$ megohm per mile, then it will be found that the charge is ·84 of the normal charge $\frac{E c l}{2}$ with perfect insulation. And if the insulation is as low as $\frac{1}{4}$ megohm per mile, the charge is only ·59 of the normal charge. The induced charges on neighbouring wires are reduced in the same proportion.

The current is

$$-\frac{v'}{k} = \frac{E}{kl} \cdot hl \cdot \frac{e^{h(l-x)} + e^{-h(l-x)}}{e^{hl} - e^{-hl}}$$

It is increased from its normal amount $\frac{E}{kl}$ in the first part of the line and reduced in the other part, but it will be found that its mean strength is still $\frac{E}{kl}$.

When the line is discharged by removing E and earthing at $x = 0$, the integral current of discharge at any point $x$ is

$$\frac{E\,c\,l}{2}\left(e^{hl}-e^{-hl}\right)^{-1}\left\{-\frac{1}{h\,l}\left(e^{h(l-x)}+e^{-h(l-x)}\right)\right.$$
$$\left.+\frac{x}{l}\left(e^{h(l-x)}-e^{-h(l-x)}\right)+2\,\frac{e^{hx}+e^{-hx}}{e^{hl}-e^{-hl}}\right\},$$

which, for perfect insulation, becomes

$$\frac{E\,c\,l}{2}\left(-\frac{2}{3}+2\,\frac{x}{l}-\frac{x^2}{l^2}\right).$$

In the line just considered, when $i = 1$ megohm, and the charge is ·84 of the normal charge $\frac{E\,c\,l}{2}$, the discharge at $x = 0$ is ·4 of the normal charge, and at $x = l$ it is only ·2 of the normal charge, the remaining ·24 passing to earth direct. And if the insulation is $i = \frac{1}{4}$ megohm, the discharge at $x = 0$ is ·32 $\frac{E\,c\,l}{2}$, and at $x = l$ it is ·06 $\frac{E\,c\,l}{2}$; and as the total charge was ·59 $\frac{E\,c\,l}{2}$, the amount of leakage is ·21 $\frac{E\,c\,l}{2}$. Thus, with $i = 1$ megohm, the leakage reduces the proportion of the discharge at the receiving end from $\frac{1}{3}$ of the original charge to less than $\frac{1}{4}$, and with $i = \frac{1}{4}$ mehogm, from $\frac{1}{3}$ with perfect insulation to about $\frac{1}{10}$.

18. Of the three sets of equations, those of continuity, those between the potentials and densities, and those between the currents and electromotive forces, only the first require to be modified to bring in leakage. As in § 7, one expression for the excess of current at $x$ over that at $x + d\,x$ is $-\gamma'\,d\,x$. The other is now $\left(\dot\rho + \frac{v}{i}\right) d\,x$, where the additional term $\frac{v}{i}\,d\,x$ is the leakage current of $d\,x$, viz., the potential of $d\,x$ divided by its insulation resistance, which is $\frac{i}{d\,x}$. Thus the equation of continuity becomes

$$\gamma' + \dot\rho + \frac{v}{i} = 0,$$

and the changes in the equations for the potential are easily found.

Equation (21), § 13, for the potential of an isolated line, now becomes

$$v'' = \left(k + s\,\frac{d}{d\,t}\right)\left(\frac{v}{i} + c\,\dot v\right) \dots \quad \dots \quad (25.)$$

First putting $s = 0$, or ignoring electro-magnetic induction compared with electro-static, which may be done for long lines, (25) becomes

$$v'' = h^2 v + c k \dot{v}$$

whose elementary solution is

$$v = A e^{-\frac{t}{c i}} \sin. (n x + b) e^{-\frac{n^2 t}{c k}};$$

thus the general solution for any arbitrary initial potential is the same as (4), § 7, if the summation there be multiplied by $e^{-\frac{t}{c i}}$.

To estimate the relative rapidity of disappearance of an assumed initial potential $A \sin. \frac{\pi x}{l}$ with and without leakage, let $l = 400$, $k = 15$ ohms, $i = 1$ megohm, as previously, and $c = \cdot 02$ microfarad. Then $c k = 3 \times 10^{-7}$ and $c i = \cdot 02$ second. Therefore

$$\frac{c k l^2}{\pi^2} = \frac{48 \times 10^{-3}}{\pi^2} = \text{about } \frac{1}{200}$$

and the rates of decay are as 250 : 200, or 5 : 4 in favour of the leakage. If $l = 800$ miles, the ratio is 2 : 1. The higher terms in the expansion for $v$ are little affected.

It is worthy of notice that if we neglect electro-static in comparison with electro-magnetic induction, an equation of the same form is obtained. Putting $c = 0$ in (25),

$$v'' = h^2 v + \frac{s}{i} \dot{v}$$

Here $c k$ has become $\frac{s}{i}$, and the solution is therefore

$$v = \Sigma A e^{-\frac{k t}{s}} \sin. (n x + b) e^{-\frac{i n^2 t}{s}}$$

But $\frac{s}{i}$ is of course very small compared with $\frac{s}{k}$, so that the potential on putting on a battery is established almost instantaneously. The rise of the current, however, depends almost entirely on the time-current $\frac{s}{k}$, and is consequently much slower.

Now, reckoning both the inductions, the elementary solution of (25) is

$$v = \sin. (n x + b) e^{-p t} (A \sin. + B \cos.) q t$$

where

$$p = \frac{k + \frac{s}{c\,i}}{2\,s}\,;\; q = \frac{k}{s}\sqrt{\frac{s}{c\,k^2}\left(n^2 + \frac{k}{i}\right) - \frac{1}{4}\left(1 + \frac{s}{c\,k\,i}\right)^2}$$

Comparing with the corresponding formulæ in § 13 without leakage, it is seen that $n^2$ becomes $n^2 + \frac{k}{i}$, and $k$ becomes $k + \frac{s}{c\,i}$. The first change is insignificant, the second of some magnitude. The rate of decay being proportional to $p$ is increased in the ratio $1 : 1 + \frac{s}{c\,k\,i}$. Taking $\frac{s}{k} = \frac{1}{1000}$, and $c\,i = {\cdot}02$, the increase amounts to 5 per cent.

For two parallel wires, the potential equations are

$$v''_1 = \left(k_1 + s_1\frac{d}{d\,t}\right)\left(\frac{v_1}{i_1} + c_1\dot{v}_1 + c_{12}\dot{v}_2\right) + s_{12}\frac{d}{d\,t}\left(\frac{v_2}{i_2} + c_2\dot{v}_2 + c_{12}\dot{v}_1\right)$$

$$v''_2 = \left(k_2 + s_2\frac{d}{d\,t}\right)\left(\frac{v_2}{i_2} + c_2\dot{v}_2 + c_{12}\dot{v}_1\right) + s_{12}\frac{d}{d\,t}\left(\frac{v_1}{i_1} + c_1\dot{v}_1 + c_{12}\dot{v}_2\right)$$

$i_1$ and $i_2$ being the insulative resistances.

If the lines are quite similar, so that their co-efficients are the same, we may use the transformation

$$v_1 = w_1 + w_2\,;\; v_2 = w_1 - w_2\,;$$

obtaining

$$w''_1 = \left(k + (s + s_{12})\frac{d}{d\,t}\right)\left(\frac{w_1}{i} + (c + c_{12})\,\dot{w}_1\right)$$

$$w''_2 = \left(k + (s - s_{12})\frac{d}{d\,t}\right)\left(\frac{w_2}{i} + (c - c_{12})\,\dot{w}_2\right)$$

which are of the same form as (25) for an isolated wire. Thus, supposing there to be two imaginary lines of capacities, $s + s_{12}$, $c + c_{12}$, and $s - s_{12}$, $c - c_{12}$, and having the *same* insulation and conductivity as the real, and without any inductive action on one another, the potential of the real lines may be expressed as the sum and the difference of the potentials of the imaginary lines.

# ON DETERMINING THE POSITION OF A FAULT WHEN BOTH ENDS OF THE CABLE ARE AVAILABLE.

By H. Kingsford.

The various corrections for Varley's Loop Test already published are in themselves sufficient proof that an error always exists in results obtained from the well-known $\frac{R - r}{2}$ formula, and a large and varied experience in fault-testing has convinced me that this error is seldom, if ever, accurately eliminated by the application of any of these correction formulæ. During the last few years I have had ample opportunities to compare the results from Varley's Loop Test, and other methods, with those obtained in a manner somewhat different from that usually employed, and the superior results invariably obtained from this mode are, I hope, adequate apology for its publication, the more so as I have never seen it employed by any but myself, and may, therefore, at least justly conclude that either it is not generally known, or that it is not sufficiently appreciated.

Instead, then, of using an even bridge, I have found it convenient to employ a dividing one (usually 1,000 to 10), in order to obtain two independent results by reversing the ends of the cable which are connected with the two ends of the bridge in the ordinary way employed in the loop test.

Let $r$ and $r'$ represent respectively the resistances unplugged in the resistance box when equilibrium is established.

$R$ = total resistance of cable in ohms.

$S$ = total length of cable in knots.

$n = \frac{c}{c'}$ the ratio of the divided, or proportionate, coils, $c$ being the resistance of the portion between one pole of the battery and the junction J of one end of the galvanometer and one end of the resistance box.

$x$ = distance of the fault in ohms from that end of the cable which is attached direct to the end of the divided coils when the resistance $r$ is unplugged.

$y$ = distance of the fault in ohms from the other end of the cable, viz., that attached to the end of the resistance box in the same experiment.

After reversing the cable, when the resistance $r'$ has to be unplugged, $x$ will now be the resistance from the end of the resistance box up to the fault, and $y$ the resistance from the end of the divided coils up to the fault.

Then $$\frac{r + R}{n + 1} = x$$

and $$\frac{r' + R}{n + 1} = y$$

The advantages possessed by this method are now at once obvious, since if $x$ and $y$ be correct their sum will equal R. This will be the case very nearly if the resistance of the fault be not excessive. It would appear also—and my observations have tended to prove this—that any error that may be denoted by $x + y - R$ may be considerably reduced, if not entirely eliminated, by calling respective distances in knots

$$\frac{\frac{x}{x + y}}{s} \text{ and } \frac{\frac{y}{x + y}}{s} \text{ instead of } \frac{\frac{x}{R}}{s} \text{ and } \frac{\frac{y}{R}}{s}$$

This will give a sufficiently accurate result where the insulation-resistance of the whole cable is uniform and infinitely superior to the resistance of the fault. But if the insulation resistance of the whole cable be low, it will be advisable to obtain results by applying to $x$ and $y$ the customary correction formula for Varley's Loop Test. The sum of these revised resistances should then equal R, otherwise the insulation resistance of the whole cable is not uniform. An error arising from this cause cannot, of course, be eliminated without a knowledge of the insulation resistance of every part of the cable, which is next to a practical impossibility.

The superiority of this method is also particularly marked

where a small resistance indicates a considerable length, as is of course the case with a large conductor. I may state, in conclusion, that in every instance that I have employed this method the result has been correct, while the results from other methods have been more or less at fault. In one case the resistance of the fault varied much and incessantly, and never fell below 120,000 ohms. These facts incline me to think that exactitude may be obtained by using this system, even where the very high resistance of the fault will render results obtained in other ways at best only approximate to the truth. I will not further intrude on your space by a detailed comparison of results which might be deemed superfluous.

# ABSTRACTS.

## F. EXNER—THE CAUSE OF ELECTRIC EXCITATION BY THE CONTACT OF DISSIMILAR METALS.

(*Annalen der Physik und Chemie, B. IX., H.* 4, *No.* 4, 1880, *pp.* 591-613.)

The author states that no satisfactory explanation of contact electricity has yet been advanced, and that he was therefore led to look for the production of electricity, not in the contact of two metals, but in previous chemical actions of the surrounding media; and he comes to the conclusion that the so-called contact electricity is produced by the excitation of the metals in contact with the oxygen of the air, in the same way as electricity is evolved by the excitation of the zinc in galvanic cells. By a kind of reasoning he arrives at the theoretical conclusion that if the heat-value of the Daniell's cell be A per chemical equivalent, and the heat of combustion of the zinc be B, then the potential difference between zinc and platinum is equal to $\frac{B}{2A}$; or if the metal connected with the zinc is oxidised in the air, the potential difference between the metals is measured by half the difference of the heats of combustion. As Professor Exner considers that since the time of Kohlrausch only isolated and untrustworthy contact experiments have been made, he repeats, himself, Kohlrausch's original experiments on the E.M.F. of contact of zinc and platinum, copper and platinum, and iron and platinum, using Kohlrausch's own method, and he obtains results which, combined with J. Thomsen's determinations of the heats of combustion, appear to prove that the potential difference between two metals in contact is really measured by half the difference of the heats of combustion. Professor Exner refers to Pfaff's early experiments, which appeared to prove that the contact electrc-motive force between two metals was independent of the gaseous medium, and he considers that De la Rive's subsequent experiments negatived that conclusion. Professor Exner describes the following experiment of his own, which he regards as proving that contact electricity is produced by the gaseous medium:—

"A short glass cylinder was closed air-tight at its upper end with a plate of silver, which did not, however, touch the glass, as there was a collar of paraffin wax. The bottom of the vertical glass tube was closed air-tight with a cork, through which passed two small glass tubes to admit the gas and allow it to escape by, and also a platinum wire well insulated by paraffin wax, and the inner end of which touched the silver plate. This latter was only for making metallic connection with the condenser.

"A second silver plate, of the same dimensions as the first, could be placed on this condenser in the same way as in the earlier experiments.

"Now, if the condenser-plates were connected, there was not the least

charge. But as soon as the interior of the glass tube was filled with dry chlorine, the condenser showed at once a considerable and quite constant electro-motive force."

The author concludes by referring to Mr Brown's experiments, published in the *Phil. Mag.*, also tending to show that the so-called contact electricity is the result of chemical action.

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## J. L. HOORWEG—HEAT THEORY OF THE PRODUCTION OF ELECTRICITY.

(*Annalen der Physik und Chemie, B. XI., H. 1, No. 9, 1880, pp.* 133-155.)

This is a continuation of the paper of which an abstract has been already given in the *Jour. Soc. Tel. Eng.*, V. IX., No. 33, 1880, p. 327, and the author now regrets that when writing his first paper he was not well acquainted with the beautiful experiments of Hankel, as was also the case with the experiments of Ayrton and Perry, which he says surpass his own in exactness, whilst they give additional confirmation to his theory. The author had already stated that "if in a closed chain of different materials the sum of the electric differences is not zero, then a galvanic current is produced," and he proceeds to examine such combinations. He refers to the various experiments which have been made on the conductivity of glass, gutta percha, paraffin wax, ebonite, resin, alcohol, &c., and to the fact that the distinction between conductors and non-conductors is one of degree, and not one of kind. He objects to the division into metallic conductors and electrolytic conductors, or into solid and liquid conductors, and shows that we can arrange all bodies in a series, beginning with the very strongly dielectric but ill-conducting airless space, and ending with the highly conducting silver. After discussing the members of this series, he divides them into two groups, the one containing those substances of which the conductivity increases with temperature, and the other bodies of which the conductivity diminishes as the temperature rises, and in reference to the statements at the beginning of his paper he concludes that "a current is always produced if at least one link of the closed chain is a dielectric of the first group." Using a galvanometer which showed a deflection of 242 scale divisions with one micro-farad current, and an electrometer with which a deflection of 20 centimetres was produced with one Daniell's cell, as well as a Lippman's capillary electrometer, he measured the currents which flowed through the following dielectrics when kept at different temperatures, and the difference of potentials at the two sides of the dielectric:—stearic acid, paraffin, spermacetti, rape oil, resin, wax, sulphur, chalk, lead chloride, dry zinc sulphate, pulverised copper sulphate, and carbon. He obtained consistent results agreeing with those of Jäger, Bonenberger obtained with dry piles, and with these obtained by Ayrton and Perry with paraffin wax, resin, ebonite, gutta percha, and mica, and he shows their connection with the phenomena observed in Sabine's selenium-water-platinum cell, and with these occurring in any two fluid voltaic elements. From the consideration that the E.M.F. of the different combinations is never

quite the same as the electric difference of zinc and copper, he draws the important conclusion that not only in the contact of metals, but also of insulators and dielectrics amongst each other, there are certain constant differences of potential. He refers to the experiments of J. Thomson on the contacts of glass, vulcanite, &c., and the friction theories of Buff and Helmholtz; and he refers to the great difficulties he himself met with when attempting to measure the contact charges of very bad conductors. After giving his results for wax, paraffin, sealing wax, resin, sulphur, stearic acid, gutta percha, paper, and glass, he shows that the contact difference of a metal with a dielectric is of the same sign as that obtained with slight friction or pressure; great friction, however, sometimes produces an opposite effect which he attributes to increase of temperature. Gases, he finds, seem to have little influence on the production of electricity.

The author refers here to his thermal theory of the galvanic current, described in the former abstract, *Jour. Soc. Tel. Eng.*, V. IX., No. 33, 1880, p. 327, and he proves that thermo-electric currents and Peltier effects are produced in the bodies referred to above. He now proceeds to show that Clerk Maxwell's reasoning for remanent charge in a compound dielectric may be applied to a homogeneous dielectric, if we assume a contact difference of potentials between the dielectric and the metal plate; and employing his thermal theory, and assuming that not only the current in the dielectric is equal to the one in the external circuit, but also that the electricity in the dielectric is changed into heat, which is reconverted into electricity in the remanent charge, he concludes that neither evaporation, liquefaction, osmose, capillarity, &c., need be regarded as sources of electricity, and that in every case the true source is contact electricity. He discusses the production of electricity by chemical action at some length, and observes that neither the severe attacks nor the beautiful experiments of Faraday have at all shaken the contact theory, and that according to Exner it is gaining an increasing number of converts. He considers Exner's crucial experiments, and shows, by referring to Ayrton and Perry's experiments, that the phenomenon observed by Exner was due to there being a cell of silver, silver chloride and platinum, or that Exner merely measured the contact electro-motive piece of silver and silver chloride. He considers that Exner's second experiment speaks as strongly against Exner's theory as the first speaks for it; and he points out that Exner's own results obtained with zinc-platinum, copper-platinum, iron-platinum, and which agree so well with Exner's heat theory, are quite different from those obtained by other experimenters, and to make this quite evident he gives the following table:—

| | Kohlrausch. | Hankel. | Exner. | Ayrton and Perry (1880). |
|---|---|---|---|---|
| Zn \| Pt ... ... ... | 0·984 | 0·984 | 0·881 | 0·981 |
| Cu \| Pt ... ... ... | 0·184 | 0·184 | 0·367 | 0·238 |
| Fe \| Pt ... ... ... | 0·384 | 0·312 | 0·704 | 0·369 |

Professor Hoorweg concludes that the only source of electricity is to be found *in contact.*

## F. EXNER—THE THEORY OF INCONSTANT GALVANIC CELLS.

(*Annalen der Physik und Chemie, B. X., H. 2, No. 6, 1880, pp. 265-284.*)

The author states that the ordinary theory of polarisation in a Smee's element is untenable, because polarisation can only take place when recombination of the products of decomposition is possible without the absorption of energy. The chemical action in a Smee's cell is the decomposition of water and the formation of an equivalent of zinc sulphate, which gives to this cell a theoretical E.M.F. of 0·732 Daniell, and according to the chemical theory no polarisation can occur, seeing that the decomposed hydrogen and the oxygen in the zinc sulphate cannot recombine without a consumption of energy, so that the electro-motive force should remain constant whatever may be the negative metal. According to the contact theory, however, the layer of hydrogen covering the negative metal ought to reduce the effective E.M.F., and so give rise to polarisation as is usually observed in Smee's cells This delusive argument in favour of the contact theory is, however, found to disappear, as careful experiments made by the author show that the E.M F. of a Smee's cell commences with a higher value than that of a Daniell's cell, and that when proper means are taken for preventing the zinc sulphate reaching the negative metal the E.M.F. sinks to 0·73 Daniell, and remains constant at this value, independent of the nature of the negative metal and of the time the cell is closed. The author enters at length into the cause of the primary large E M.F. of the Smee's cell, and shows that it arises from the oxygen dissolved in the liquid, and which is able to combine with the zinc without the absorption of energy necessary for the decomposition of the equivalent of water. The effects of dissolved gas has also been observed by De Fonville and others. Before describing his own experiments, the author states the problem to be solved is as follows:—

"The contact theory requires that the E.M.F. of a Smee's element should begin with a value 0·73 of a Daniell's cell," [this is derived from the heat of combustion of zinc in oxygen, the combination of the oxide with sulphuric acid, minus the energy required for the decomposition of an equivalent quantity of water] "and then sink to a lower value, which depends on the amount of polarisation, that is, on the amount of resistance; and, further, that it depends on the nature of the negative plate. The chemical theory, on the contrary, requires that the Smee's element should first have an E.M F. between 0·732 and 2·15 of that of a Daniell" [2 15 is derived from only considering the heat of combustion of zinc with oxygen and the combination of the oxide with sulphuric acid] "and that this value should fall to 0·732 of a Daniell, and then remain perfectly constant, no matter what be the metal forming the negative plate, as long as it does not give rise to chemical changes. Further, the value 0·732 of a Daniell must also depend on the resistance of the element."

He employed a cell in which deposition of the zinc on the negative plate was rendered impossible by having the plates in separate vessels connected with a glass tube drawn out to a fine point, and be found that the primary E.M.F. was 1·15 that of a Daniell's cell, and that it diminished after a current produced

by short-circuiting has deposited hydrogen on the platinum plate; also that, whatever was the negative plate, there was the same working electro-motive force "The preceding results," he concludes, "connect themselves intimately with the consequences of the chemical theory; they contradict entirely the contact theory."

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## W. BEETZ—ON THE NATURE OF GALVANIC POLARISATION

(*Annalen der Physik und Chemie, B. X., H. 3, No. 7, 1880, pp.* 348-371.)

The quadrant electrometer enables certain old and well-known simple experiments to be more accurately performed, and Exner's work during last year has been of this kind. Wiedemann's Lehrbuch gives a complete account of what was known till lately of the E.M forces of cells as depending on heats of combustion. Mr. Exner's results are welcome, and he is wrong to suppose that they contradict the theory held until now. The author quotes from former papers of his own, and mentions Schonbein (*Pogg. Ann.* 1838), pointing out that Exner's views about polarisation are already well known; that the author thirty years ago, and more lately Crova, Bosscha, and others, pointed out, as Exner now does, the influence of the formation of hydrogen suboxide, attributing the different results of different observers to imperfect observation of the minor circumstances of electrolysis, size of plates and so forth, and the influence of the amount of the decomposing E.M.F. Crova, in 1863, said: "As long as the E.M F. of the cell producing the currents is less than a certain limit, the final E.M.F. of the polarisation is equal to that of the cell, and increases with it from zero to this limit. If the E.M.F. surpasses this limit, then the production of gas begins on the surface of the plates." Crova also gives Exner's statement, that if one part of a closed chain of substances is an electrolyte, this becomes decomposed as soon as an E.M.F. becomes active in the chain. This statement has generally been received as correct, but it required Exner's confirmation. The author now proceeds to describe at great length his experiments, the results of which show that the whole polarisation is due to the sum of the separate effects at the two electrodes, a theory opposed by Exner, who thinks that hydrogen on one platinum plate has no polarisation effect unless there is oxygen in the liquids or on the other platinum plate with which it can combine, and Beetz satisfies himself that in disproving Exner he has taken as great care as it is possible to take in the removal of gases from the plates and liquids.

The author now goes on to consider the differences between Exner and himself as to the polarisation in the simple cell. Exner says that the true E.M F. of a zinc-platinum dilute sulphuric acid cell is 0·73 Daniell, and that the high primary E.M.F. which is observed is due to oxygen in the liquid and on the platinum plate. The other view is that the primary E.M.F. is the true one, which is diminished by the production of hydrogen. Beetz gives the results of his experiments made 31 years ago, to show that Exner's view is really the same as the other.

His experiments with other negative plates than platinum do not confirm those of Exner, who found with a copper plate the same E.M.F. as with a platinum plate. Beetz is sure that in his experiments no zinc was deposited on the negative plate during the three minutes' closure. He gives the following E.M. forces in terms of a Daniell:—

| Zinc-Platinum. | | Zinc-Copper. | | Zinc-Silver. | |
|---|---|---|---|---|---|
| Open. | Closed. | Open. | Closed. | Open. | Closed. |
| 1·52 | 0·72 | 0·98 | 0·46 | 1·23 | 0·51 |

Using sodium amalgam as his positive plate, he found—

| Na—Pt. | | Na—Ag. | | Na—Cu. | | Na—Zn. | |
|---|---|---|---|---|---|---|---|
| Open. | Closed. | Open. | Closed. | Open. | Closed. | Open. | Closed. |
| 2·31 | 1·33 | 2·05 | 1·22 | 1·79 | 1·14 | 0·78 | 0·68 |

These numbers confirm the law of the tension series extended to combinations of metals and fluids. The differences between the E.M. forces of a closed sodium-platinum and a closed sodium-zinc element is so great, that one cannot conceive an accidental reason for the change. The author then proceeds to consider whether, when zinc is the negative plate, it does not oxidise like the sodium, but this is not the case; and he says that he is quite unable to explain the great differences which he has observed with different negative metals. He describes at some length his experiments, which, he says, contradict decidedly Exner's views of polarisation, and he maintains the truth of the commonly understood theory of polarisation, inasmuch as researches such as that of Kohlrausch on the E.M.F. of very thin layers of gas have led to results which agree well with experience. The author says that it is very wrong with our present information to begin again the quarrel of the chemical and contact theories. Even if Exner explains Volta's experiments, the fundamental ideas of the origin of the current held since the time of Ohm will not be destroyed, they will only be explained; "only I cannot agree that the material which is before us is sufficient in order to represent the known phenomena of galvanism so simply as purely chemical processes."

## W. E. AYRTON AND JOHN PERRY—NOTE ON PROFESSOR EXNER'S PAPERS ON CONTACT ELECTRICITY.

(*Philosophical Magazine, Vol. II., No.* 65, 1881, *pp.* 43-54.)

The authors refer to Professor Exner's papers, abstracts of which precede this, and they show that Exner's calculation of the observed contact E.M.F. from the heats of combustion have nothing in common with the calculations

of the E.M.F. of cells made in 1851 by Sir William Thomson from their mechanical effects. They prove that Professor Exner's hypothesis that "a piece of zinc through oxidising in the air has a potential + E, and the oxidised layer, or rather the layer of air which is in contact with it, has on the contrary a potential + E, so that the potential difference is equal to 2 E, which must be measured by the heat of combustion of the zinc," does not mean either that there is a constant difference of potential 2 E, in accordance with the modern mathematical language of electricity, nor does the above statement mean that there are definite charges of electricity + E and — E; and they show that Professor Exner's *a priori* conclusion that the potential difference between two metals in contact is measured by half the difference of the heats of combustion, although apparently based on reasoning from first principles, is in reality simply an assumption which Professor Exner has chosen to make, and which could not have been arrived at by logical deductions from the known laws of electricity. They next consider how far Professor Exner's experiments on the E.M.F. of contact of zinc and platinum, copper and platinum, and iron and platinum, made with Kolrausch's original method, and which, when combined with J. Thomsen's heats of combustion, appear to substantiate in a marked way Professor Exner's theory, can be regarded as an experimental proof of this theory, which otherwise would be without proof; and they draw attention to the table given in Professor Hoorweg's paper on the "Heat Theory of the Production of Electricity" (an abstract of which also precedes this), which shows that these results of Professor Exner's, which accord so well with the numbers required by his theory, are in discord with those obtained by Kolrausch, Hankel, and themselves, which latter agree fairly well among one another, considering that these experimenters used different methods in their investigations, and necessarily different specimens of the metals experimented on.

With reference to the second of Professor Exner's papers, abstracted in this number of the *Jour. of the Soc. of Tel. Eng.*, they think that the ideas contained in it are based on some misconception of the contact theory of electricity. They point out that Professor Exner is mistaken in stating that "the contact theory requires that the E.M.F. of a Smee's cell should begin with a value 0·73 that of a Daniell's cell," and they observe that what the *summation law* of the contact theory states is that the E.M.F. of a complete cell is equal to the algebraical sum of all the differences of potential, each measured separately at the separate contacts of metals with any layer of gas on them, gas with liquid, one liquid with another, &c., and that when this law is applied to the results of their own contact experiments it gives a value of about 1·270 volts as the primary E.M.F. of a Daniell's cell. They quote fully from Sir Wm. Thomson's paper of 1851 to show that he quite anticipated Professor Exner in all that he says about the effect of dissolved gas on the liquid causing the Smee's cell to have an initial E.M.F. greater than that of a Daniell's cell; and they prove that Professor Exner is quite wrong in thinking the contact theory requires that the *working* E.M.F. of a Smee's cell must necessarily depend on the negative metal, for they observe that when the negative plate becomes sufficiently coated with hydrogen

the contact between the then conducting layer of gas and the metal is almost like that of a pair of metals, so that

$$H \mid Pt + Pt \mid Zn = H \mid Cu + Cu \mid Zn$$

or any negative metal would eventually act like a conducting hydrogen plate.

The authors point out that between the contact theory, as properly understood, and the chemical theory there is nothing antagonistic, that the E.M.F. in any circuit can be calculated either on the contact theory, if we know *each of all* the differences of potential at the various surfaces of separation of dissimilar substances (solid, liquid, or gaseous) in the circuit; or it may be calculated on the chemical theory if we know exactly what are *all* the physical and chemical changes taking place, and the heat equivalent of every one of them, and that the amount of the E.M.F. determined in either of these two ways must be identical. They show, however, that as measurements with an electrometer are far more sensitive than those made with a chemical balance, the contact method of determining an electro-motive force is probably far more delicate than the chemical one.

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## F. GUTHRIE AND C. V. BOYS—ON MAGNETO-ELECTRO INDUCTION. PART II.—CONDUCTIVITY OF LIQUIDS.

(*Philosophical Magazine, Vol. X, No.* 63, 1880, *pp.* 328-337.)

A conductor in a moving magnetic field is urged to move by a force varying as the product of the conductivity into the relative speed, so that by observing the torsion of a wire supporting successively different substances of the same form and size in a revolving magnetic field, a measure of their relative conductivity may be obtained. This method seemed specially applicable to electrolytes, owing to the absence of electrodes, electrolysis, and polarisation. The apparatus employed consisted of a glass shade containing one litre of liquid suspended by ebonite strips to a horizontal boxwood beam, which was hung to a long thin steel wire. Completely surrounding the vertical circular sides of the vessel is a powerful magnet, consisting of 24 semicircular bars; a remarkably uniform field of force through the liquid is produced. The magnet is fixed to the top of a vertical steel shaft beneath the vessel, and the shaft (and with it the magnet) is made to revolve with great rapidity by a band driven by steam power. To protect the glass vessel, etc., from the whirlwind caused by the revolving magnet, a screen is interposed between them and the magnet. The speed of the latter is measured by the number of turns per second as indicated by a wheel turning once for 10,000 of the magnet, and a bell striking at every 100th turn. When the magnet is revolving rapidly, say 3,000 turns per minute, the liquid experiences a force tending to turn it with the vessel in the same direction, but the vessel comes to rest from the torsion of the wire, and the motion of the liquid is checked by the friction between its cylindrical layers, and between it and the glass, so that its actual revolving motion is very slow, giving rise to no appreciable error (only 1 in 20,000), and the friction is balanced by the torsion of the suspending wire, so that the deflection of the

vessel is a measure of the force between the magnet and the liquid, and therefore of its conductivity. The deflection is observed by a microscope directed towards a scale fixed to the vessel.

The practical difficulties appear to have been considerable, and arose mainly from the variation of steam pressure, and from the impossibility of finding the zero of the scale, owing to the position of equilibrium of the vessel being different for different positions of the magnet when at rest. It was therefore necessary to make observations of the deflections at different speeds, together with an observation when a disc of brass was suspended from the boxwood beam, and combine them so as to eliminate the unknown factors. The only substances experimented on were sulphuric acid and sulphate of copper, the results for which are given. The conductivity curve for the former agreed substantially with Kohlrausch's, who used alternating currents. There is a sharper rise to and fall from the first maximum, otherwise the position of the two maxima, of the minimum, and of the point of contrary flexure agreed most perfectly.

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## JULIUS THOMSEN—THE CHEMICAL ENERGY AND THE ELECTROMOTIVE FORCE OF DIFFERENT GALVANIC COMBINATIONS.

(*Annalen der Physik und Chemie*, *B. XI.*, *H. 2*, *No.* 10, 1880, *pp.* 246-269).

It has hitherto been difficult to determine whether the whole or only a portion of the energy produced by chemical action is converted into electricity for want of sufficiently accurate measurements of the heat corresponding with the chemical reactions. The author is now able to solve this problem, using his recently thermo-chemical results. The first condition that all chemical energy can be converted into electricity is that no chemical action must take place when the circuit is broken, a condition realised in the so-called constant batteries The current from a Daniell's cell was made to pass through a calorimeter containing four platinum spirals, and the quantity of heat produced by the current measured, the current itself being also measured on a sine galvanometer, the absolute value of the deflections of which were known by a previous comparison having been made with the corresponding quantities of gas given off in a voltameter. The resistance of the Daniell's cell was also determined so that the total quantity of galvanic heat produced in a given time could be calculated. Knowing the absolute value of the current flowing during this time, the amount of chemical decomposition going on in the Daniell's cell could be calculated, and its heat equivalent determined from the author's recent experiments on the heats of combustion. And it was found that in the case of a Daniell's cell the galvanic heat or heat generated by the current flowing through resistance was practically equal to the whole of the heat that would have been produced by the chemical decomposition in the cell had the chemical reactions taken place without production of electricity, the two numbers being 50,292 in the first case and 50,130 in the second, or practically the whole of the chemical energy in this case is converted into heat.

The author next compares the chemical energy and the E.M.F. of different galvanic combinations, and the results are shown in the following table:—

| | Galvanic Combination. | Its Chemical Reaction. | Corresponding production of Heat. | Energy of the Combination. Absolute. | Energy of the Combination. Plus of Daniell's Cell as unit. | Name of the Inventor. |
|---|---|---|---|---|---|---|
| 1 | Zinc ... ... ...<br>Sulphuric acid ... | + (Zn, O, $SO_3$ Aq) | +106,090c. | 50,130c. | 1·00 | Daniell |
| | Sulphate of copper<br>Copper ... ... ... | — (Cu, O, $SO_3$ Aq) | — 55,960c. | | | |
| 2 | Zinc ... ... ...<br>Sulphuric acid ... | + (Zn, O, $SO_3$ Aq) | +106,090c. | 16,590c. | 0·33 | Regnauld |
| | Sulphate of Cadmium<br>Cadmium ... ... | — (Cd, O, $SO_3$ Aq) | — 89,500c. | | | |
| 3 | Zinc ... ... ...<br>Hydrochloric acid | + (Zn, $Cl_2$, Aq) ... | +112,840c. | 54,080c. | 1·08 | Pincurs |
| | Chloride of silver...<br>Silver ... ... ... | — ($Aq_2$, $Cl_2$ ) ... | — 58,760c. | | | |
| 4 | Zinc ... ... ...<br>Sulphuric acid ... | + (Zn, O, $SO_3$ Aq) | +106,090c. | 96,080 | 1·92 | Bunsen |
| | Nitric acid hydrate<br>Carbon ... ... ... | — ($N_2$ $O_4$, O, $H_2$ O) | — 10,010c. | | | |
| 5 | Zinc ... ... ...<br>Sulphuric acid ... | + (Zn, O, $SO_3$ Aq) | +106,090c. | 82,810 | 1·65 | Bunsen |
| | Dilute nitric acid...<br>Carbon ... ... ... | — $\frac{1}{3}$($N_2O_2$,$O_3$, 7 $H_2O$) | — 23,280c. | | | |
| 6 | Zinc ... ... ...<br>Sulphuric acid .. | + (Zn, O, $SO_3$ Aq) | +106,090c. | 99,790 | 1·99 | Bunsen |
| | Chromic acid ...<br>Carbon ... ... .. | — $\frac{1}{3}$ ($Cu_2$ $O_3$, $O_3$, Aq) | — 6,300c. | | | |
| 7 | Copper ... ... ...<br>Sulphuric acid ... | + (Cu, O, $SO_3$ Aq) | + 55,960 | 45,950 | 0·92 | Thomsen |
| | Nitric acid hydrate<br>Carbon ... ... .. | — ($N_2$ $O_4$, O, $H_2$ O) | — 10,010 | | | |
| 8 | Iron... .. ... ..<br>Hydrochloric acid | + ($Fe_2$, $Cl_2$, Aq) ... | + 99,950c. | 44,430 | 0·89 | Ponci |
| | Perchloride of iron<br>Carbon ... ... ... | —($Fe_2$ $Cl_4$ Aq $Cl_2$ ) | — 55,520c. | | | |
| 9 | Zinc ... ... ...<br>Sulphuric acid ... | + (Zn, O, $SO_3$ Aq) | +106,090c. | 37,730 | 0·75 | Wollaston & Siemens |
| | Platinum ... ... | — ($H_2$, O) ... ... | — 68,360c. | | | |

Lastly, the author compares the calculated E.M.F with the actual electromotive force, and the amount of accordance of the numbers given in the last

two columns of the following table shows what proportion of the total energy of the cell is converted into electricity.

| | Combination. | Chemical Energy. | | E.M.F. |
|---|---|---|---|---|
| | | Absolute. | Relative. | |
| 1 | $\left\{\begin{array}{l}Zn—H_2SO_4 + 100 H_2O\\Cu—conc.\ CuSO_4 Aq\end{array}\right\}$ ... .. ... | 50,130 | 1 | 1 |
| 2 | $\left\{\begin{array}{l}Zn—H_2SO_4 Aq\\Cd—conc.\ CdSO_4 Aq\end{array}\right\}$ ... ... ... | 16,590 | 0·33 | 0·33 |
| 3 | $\left\{\begin{array}{l}Zn—HCl Aq\\Ag—AgCl\end{array}\right\}$ ... ... ... .. | 54,080 | 1·08 | 1·065 |
| 4 | $\left\{\begin{array}{l}Zn—H_2SO_4 + 100 H_2O\\C—HNO_3\end{array}\right\}$ ... ... | 96,080 | 1·92 | 1·86 |
| 5 | $\left\{\begin{array}{l}Zn—H_2SO_4 + 100 H_2O\\C—HNO_3 + 7 H_2O\end{array}\right\}$ ... ... | 82,810 | 1·65 | 1·69 |
| 6 | $\left\{\begin{array}{l}Cu—H_2SO_4 + 100 H_2O\\C—CrO_3,\ SO_3 Aq\end{array}\right\}$ ... ... | 99,790 | 1·99 | 1·85 |
| 7 | $\left\{\begin{array}{l}Zn—H_2SO_4 + 100 H_2O\\C—HNO_3\end{array}\right\}$ ... ... | 45,950 | 0·92 | 0·88 |
| 7b | $\left\{\begin{array}{l}Cu—H_2SO_4 + 100 H_2O\\C—HNO_3 + 7 H_2O\end{array}\right\}$ ... ... | 32,680 | 0·65 | 0·73 |
| 8 | $\left\{\begin{array}{l}Fe—FeCl_2 Aq\\C—Fe_2Cl_6 Aq\end{array}\right\}$ ... ... ... ... | 44,430 | 0·89 | 0·90 |

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## J. H. LONG—THE ELECTRIC CONDUCTIVITY OF SOLUTIONS OF SALTS.

*(Annalen der Physik und Chemie, B. XI., H. 1, No. 9, 1880, pp. 37-46.)*

In continuation of the researches of Kohlrausch (Wied. Ann. 6., p. 1, 1879), Mr Long gives the results of a number of experiments he has made on the electric conductivity of the solutions of various salts. For measuring the resistances, Mr. Long used Kohlrausch Wheatstone's bridge method, replacing, however, the galvanometer with a telephone. The author gives tables for $MnCl_2$, $ZnCl_2$, $CuN_2O_6$, $SrN_2O_6$ and $PbN_2O_6$ showing the quantity of salt in the solution, the specific gravity of the latter, the temperature, the conductivity and the constants in the formula $k_t = k_0 (1 \times \alpha t \times \beta t^2)$ for calculating the conductivity of the same solution at other temperatures. In the following table the third column gives the number of molecules set free, calculated from the formula $m = 1000 \frac{p s}{A}$, where $p$ is the weight of the electrolyte contained in the unit of weight of the solution, $s$ the specific gravity of the solution, and A the weight of the electro-chemical equivalent of the substance set free. The fourth column gives the conductivity at 18° C., and the fifth the increase of the conductivity for 1° C. in the neighbourhood of 22° in terms of that at 18°.

| Per Cent. | Specific Gravity at 15° C. | Number of Molecules 1,000 × $m$. | $10^8 \times k_{15}$ | $\frac{\Delta k}{k_{15}}$ |
|---|---|---|---|---|
| | | Mn | $Cl_2$ | |
| 5 | 1·0456 | 830 | 492 | 0·0210 |
| 10 | 1·0895 | 1,729 | 790 | 0·0206 |
| 15 | 1·1378 | 2,709 | 987 | 0·0202 |
| 20 | 1 1900 | 3,778 | 1,061 | 0·0206 |
| 25 | 1·2472 | 4,949 | 1,020 | 0 0203 |
| 28 | 1·2828 | 5,701 | 950 | 0·0208 |
| | | Zn | $Cl_2$ | |
| 2·5 | 1·024 | 376 | 258 | 0·0213 |
| 5 | 1·048 | 771 | 452 | 0·0192 |
| 10 | 1·094 | 1,609 | 680 | 0·0165 |
| 20 | 1·190 | 3,500 | 853 | 0·0156 |
| 30 | 1 299 | 5,731 | 866 | 0·0172 |
| 40 | 1·423 | 8,371 | 790 | 0·0198 |
| 50 | 1 570 | 11,540 | 589 | 0·0232 |
| 60 | 1·746 | 15,406 | 345 | 0 0307 |
| | | Cu $N_2$ | $O_6$ | |
| 5 | 1·043 | 556 | 341 | 0·0221 |
| 10 | 1·089 | 1,162 | 595 | 0·0215 |
| 15 | 1 139 | 1,822 | 808 | 0·0206 |
| 20 | 1·193 | 2,545 | [redacted] | 0·0205 |
| 25 | 1 248 | 3,328 | 1,019 | 0·0216 |
| 35 | 1·377 | 5,141 | 993 | 0·0237 |
| | | Sr $N_2$ | $O_6$ | |
| 5 | 1·0418 | 492 | [redacted] | 0·0225 |
| 10 | 1·0857 | 1,026 | 493 | 0·0225 |
| 15 | 1·1318 | 1,605 | 645 | 0·0227 |
| 20 | 1·1815 | 2,234 | 750 | 0·0228 |
| 25 | 1·2363 | 2,922 | 810 | 0·0226 |
| 35 | 1·3542 | 4,481 | [redacted] | 0·0241 |
| | | Pb $N_2$ | $O_6$ | |
| 5 | 1·0449 | 316 | 179 | 0·0238 |
| [redacted] | 1·0937 | 661 | 301 | 0·0251 |
| 15 | 1·1467 | 1,039 | 401 | 0·0251 |
| 20 | 1·2043 | 1,455 | 487 | 0·0250 |
| 25 | 1·2678 | 19,15 | 561 | [redacted] |
| 30 | 1·3358 | 2,421 | 625 | 0·0257 |

The author has also tried the 20 per cent. Zn $Cl_2$ solution with Kohlrausch and Grotian's dynamometer method, and obtained the result

$$k_t = 591\ (1 \times 0{\cdot}0257\ t - 0{\cdot}000069\ t^2).$$

On comparing his results with those of Lenz for $Zn\,Cl_2$ and with those of Freund for $Cu\,N_2\,O_6$ he finds considerable discrepancies which he attributes to mistakes in the previous investigations of these experimenters. A graphical representation of the connection existing between $m$, as calculated from $m = 1000\,\frac{p\,s}{A}$, and the corresponding values of $10^8\,k_{18}$ and of $\frac{\Delta k}{k_{18}}$ for the various salts used is appended to the paper.

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## F. NARR—THE BEHAVIOUR OF ELECTRICITY IN GASES ESPECIALLY WHEN RARIFIED.

(*Annalen der Physik und Chemie, B. XI., H.* 1., *No.* 9, 1880, *pp.* 155–163.)

A hollow brass ball of internal diameter of 16 centimetres had a tapered hole in it 3·5 centimetres in diameter. This was closed tightly with a well-fitting plug, through which passed a platinum wire well insulated from the plug, and supporting a small metal ball in the centre of the hollow one. The other end of this wire was attached to a sine electrometer, and the outer ball rested on a green glass cylinder, which the author states was well insulated. The small ball, wire, and sine electrometer were then charged, after the gas, consisting either of air, hydrogen, or carbonic acid, had been pumped out of the hollow ball with a mercury pump, and it was found that on connecting the latter with the earth the charge in the small interior ball disappeared. If the hollow ball, however, were insulated, the charged leaked away exactly as if the space were filled with gas. The author gives values for $Q_0$ and $p$ in the formula $Q = Q_0\,c^{-pt}$ for insulated vacua formed from air, hydrogen, and carbonic acid. If the space were filled with gas, then little change was produced in the charge of the small ball when the large ball was connected with the earth, or not, provided it was connected also at the moment of charging the small one. The author states that the phenomena observed with the vacuum were not due to mere surface leakage between the platinum wire and the outer ball.

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